

# HARVARD THEOLOGICAL REVIEW

VOLUME XXI

JANUARY, 1928

NUMBER 1

## THE SERÂBÎT INSCRIPTIONS

### I. THE REDISCOVERY OF THE INSCRIPTIONS

KIRSOPP LAKE AND ROBERT P. BLAKE

HARVARD UNIVERSITY

IN the spring of 1927 a small expedition, consisting of Professors Lake and Blake, Mrs. Blake, and the Reverend A. W. Johnson, went to Mt. Sinai to study the manuscripts in the Monastery of St. Catherine. Some results relating to our more immediate aim will be published later, but the most important outcome was entirely foreign to the original plan.

In passing through Cairo we met Dr. Alan H. Gardiner, an old friend of Mr. Lake, and he suggested that on our return from the monastery we might try to bring back the inscriptions near the ancient mines opposite the temple of Hathor on Mt. Serâbît el-Khâdem. We undertook to do what we could in the matter, and consulted Mr. Wallace of the Egyptian Frontiers' Commission as to the best method of accomplishing our purpose. Mr. Wallace passed us on to the Department of Mines, and it in turn to the manager of the Sinai Mining Company, which extracts manganese from a mountain about half-way down the Gulf of Suez, a few miles from the little port of Abu Zenima. The manager, Mr. Paull, was extremely friendly, and promised that when the time came he would help us with camels and guides.

Briefly told, the story of the inscriptions is this. In 1904-05 Sir W. M. Flinders Petrie went to Serâbît and among other things found at the entrance of one of the mine-workings some fragments of inscriptions in a character which resembled Egyptian but unfortunately was meaningless when interpreted as Egyptian.

Petrie gave an account of his expedition in his *Researches in*

*Sinai*, London, 1906, but the inscriptions were published by Alan H. Gardiner and T. Eric Peet in *The Inscriptions of Sinai*, London, 1917. This volume gave first all the Egyptian inscriptions on Serâbît (nos. 1-344), and then those "in a new foreign script" (nos. 345-355). Of these, nos. 345-347 came from the Temple of Hathor; and 345 (a sphinx) is now in the British Museum (41748), 346 (an inscribed statuette) in the Egyptian Museum at Cairo (38268), 347 (another statue) in the Musée du Cinquantenaire at Brussels. No. 348 had been seen by Palmer in 1869 in the Wady Maghareh, some twenty miles from Serâbît, and was first published by R. Weill in his *Inscriptions Égyptiennes du Sinai*, Paris, 1904, from a squeeze taken by Palmer. The others, nos. 349-355, are on fragments of stone seen by Petrie at the entrance to Mine L on Mt. Serâbît.

Petrie himself offered no further suggestion as to the language of the inscriptions beyond stating that it was not Egyptian, but while Gardiner was engaged in preparing his Corpus, the thought struck him that possibly it was Semitic and represented the evolution of Egyptian hieroglyphs into a Semitic alphabet. He imagined that, if so, the principle of 'acrophony' had probably been applied. Acrophony is the principle followed when hieroglyphs (representing things) are converted into signs (representing sounds) by giving to each hieroglyph the value of the initial consonantal sound in the name of the thing represented. Obviously this conversion will give different results when different languages are used. Thus, for example, a picture of a house would mean 'h' in English but 'm' in French. It was in fact the Egyptian representation of a house which first caught Gardiner's eye. If the language was Semitic, this would mean 'b' by the principles of acrophony, since 'beth' is Semitic for house. Following up this clue he found the word 'Ba'alat' in several places in these inscriptions — and a word meaning goddess seemed, to say the least, not inappropriate in a place just opposite the temple of Hathor.

Gardiner published his results in an article, 'The Egyptian Origin of the Semitic Alphabet,' in the *Journal of Egyptian Archaeology*, III, 1917 (1916). To it Dr. A. Cowley, Bodley's Librarian at Oxford, added another article with the same title



in which he identified more letters and deciphered several words.

The results of Cowley and Gardiner were immediately taken up by Professor Sethe, now of Berlin, who had already restated the case for an Egyptian origin of the Semitic alphabet, and he made progress in identifying the letters in a series of articles to which detailed reference is made by Professor Butin in the article which follows.

Nevertheless the matter might have remained no more than one of great importance indeed to specialists, but not interesting to the public at large, had it not been for a much more ambitious decipherment by Professor H. Grimme of Münster, who maintained in his *Althebräische Inschriften vom Sinai* that he had discovered in these inscriptions a reference to Moses and to the Egyptian princess who rescued him from the Nile. This attracted the attention of the public, and the inscriptions became famous in the newspapers as the "Moses-inscription."

Sethe was not inclined to accept Grimme's reading, and a lively controversy was carried on between the two professors. Important contributions were made by other scholars, as mentioned in the article by Professor Butin. The general result was to show that no final conclusion could be reached until more accurate information was available about the fragments which had remained on Mt. Serâbît. Grimme had made use of Petrie's photographs, prints of which were sent him by Gardiner, but there was no agreement among the learned as to the decipherment of them. It is unfortunately not always possible in a photograph to distinguish marks on the stone from letters, and for this reason, even when photographs are taken under the most favorable circumstances, it is always desirable to make a transcript.

There had been some suggestion of an expedition headed by Professor Grimme to study these fragments, but when we were in Cairo it seemed doubtful whether the expedition would be undertaken, or, if it were, whether it would bring back the fragments to the Museum; for this reason Gardiner was anxious that we should do what we could while we were in the neighborhood of Serâbît. It should however be noted that 'in the

neighborhood' is a relative matter, for, stated in terms of time instead of space, the monastery was about as far from Serâbît as New York is from San Francisco.

M. Lacau, the director of the Cairo Museum of Antiquities, was sympathetic to our scheme, gave us the necessary authorization to move the fragments, and undertook to pay for their transport to Cairo. Thus when we left the monastery the way was clear for us, provided we could find the place. The journey had to be made by camel, and the Arabs who furnish camels to the monastery did not know exactly where the place was. We therefore went somewhat out of our way to Um Bogma, where are the manganese mines of the Sinai Mining Company. This took four days. Mr. Paull had left Sinai, but we found that Mr. Hoops, the acting manager of the company, had found a camel-owner who spoke French and a guide who some years before had been to Serâbît with the Geological Survey and claimed to know all the paths to and on the mountain. He proved entirely competent and was of the greatest assistance. From Um Bogma therefore we went along the Wady Shellal to the tomb of Sheik Hashash in the Wady Sahu, where there is water. Thence we crossed over into the Wady Lihyan and descended this to its junction with the Wady Suwig, into which we turned and swung southeastward. Finally on the end of the second day after leaving Um Bogma we reached the bottom of a small wady leading up into the heart of Mt. Serâbît, which we had completely skirted on three sides. From this point there was a track to the top, reasonably easy on foot but impossible for camels. The guide said that we could reach the summit in about two hours and that there was another path, constructed in part by Petrie, which made a wide circle but would bring the camels to the top in not less than six hours. We were already a day and a quarter away from the nearest supply of water, the Bir Naşb; so we pitched our camp at that point and went up on foot, sending some camels back for water. The guide took us up to the temple of Hathor, where we saw a number of Egyptian inscriptions. From this point the mine which Petrie described was only a few hundred yards away in an air-line, but was separated from us by a deep



ravine so that it took us more than an hour to walk round to the mines themselves. We had expected, if we were successful at all, to have an exciting search, but when we were still twenty yards from the mine Mr. Blake suddenly exclaimed that he could see the inscriptions. He proved to be right. Petrie had collected the smaller fragments into a heap on top of the largest piece. We thought we had found them all, but it proved afterward that one of our fragments was not in Petrie's list. It is a pity that we could not identify the fragments more accurately, but the temperature in the shade was over 115° Fahr., and the fragments were in the sun and almost too hot to touch.

While the two older members of the expedition were looking at the fragments and considering how to deal with the largest piece, which was too heavy for a camel, Mr. Johnson had been investigating the workings of the mine and before long discovered two more inscriptions inside the workings. One was just inside Mine L, and it was possible to obtain a photograph by making a very long exposure. The other was in almost complete darkness in Mine M, and was discovered only because, in feeling his way through the workings, Mr. Johnson put his hand on it and recognized from the feeling that it might be an inscription. An electric torch confirmed his suspicion. The position of the inscriptions on the rock of the mine-workings was such that it was impossible to think of cutting them out with the primitive tools at our disposal. We therefore made transcripts of both these inscriptions, Mr. Blake copying and the others separately comparing his transcript with what they themselves saw until at last all three accepted the transcript as an accurate account of what was visible. It may be asked why we did not take a squeeze. The egyptologists in Cairo had warned us not to do so, as the chemical composition of the rocks was such that water might have a disastrous effect on the surface. The rock at Serâbît is a soft sandstone so friable that it can be crumbled in the fingers. Fortunately for archaeologists the surface hardens somewhat on exposure to the air. We could not stay very long on the top of the mountain and went back to our camp for the night.

The next day we went up again, taking with us an Arab who

knew something of stone-cutting, and ordered three camels to go round and meet us at the top of the mountain as soon as they could get there. As soon as we began to move the biggest fragment, a hitherto latent crack appeared and the stone divided into two parts, and that not in the place where we wanted it to, but across one of the two inscriptions that it contained. The split in the stone, which was quite inevitable, did no serious harm, as the fragments of course fit one another, and we had no further difficulty in dividing it so as to get rid of waste stone and bring the two fragments into a form which rendered camel-transport possible. It should be noted that these two inscriptions were carved upside down relatively to each other. We had sent up with the camels all the blankets of the expedition and in these we wrapped up the fragments of stone and loaded them on the camels to go back to the camp, though not without some misgivings as to the roughness of the road. Our misgivings were not wholly unfounded, for one of the camels turned a somersault on the way down. He was not injured, but one of the fragments split in two. Fortunately it was a clean split and of no archaeological importance.

After this it was comparatively easy to ride back to the head of the railway of the mining company, where we left our stones for a day while we went to Um Bogma. Altogether it had taken us eleven days since we left the monastery, and the temperature had been very high. We were very dirty and glad of a rest. After luxuriating for a day in the hospitality of Mr. and Mrs. Hoops, we went down in the little railway of the Sinai Mining Company to Abu Zenima. Here the Company has a charming bungalow for visitors, where we stayed. We packed the fragments in wooden boxes, and after two days' rest embarked on the 'Argyll,' a small steamer plying for the use of the Company between Abu Zenima and Suez. At Suez we were met by the truck of the University of Michigan, and the next morning had the pleasure of handing over the fragments to M. Lacau at the Egyptian Museum. Thus all Petrie's fragments are now at Cairo except Gardiner's 355, and in addition there is a new fragment, which Professor Butin proposes to number 356. Similarly, the long inscription in Mine L is to receive the num-



ber 357, and the short one in Mine M that of 358. These last two of course remain at Serâbît.

Our programme then took us to Jerusalem. We stayed at the American School of Oriental Research, where fortune favored us since we found, as the acting director of the School, Professor R. F. Butin of the Catholic University of America, Washington, D. C. We were delighted at the opportunity of submitting our photographs and transcriptions to a trained Semitic scholar. Using these he was enabled, as it seems to us, to advance further and more securely than anyone else has done in deciphering and translating the inscriptions. He has since visited Cairo to see the fragments and further correct his reading, and his results are published in the succeeding article in the present number of the REVIEW.

Professor Butin's results speak for themselves, but the original members of the expedition may perhaps point out certain facts which appear to them to be significant.

1. As was mentioned above, the two originally conjoint inscriptions (nos. 351 and 353) were carved upside down relatively to one another. This is significant as proving that this large stone had not fallen from the face of the rock, as Petrie suggested, but was engraved after it had been detached. It is incredible that anyone ever wrote an inscription upside down on a vertical stele, or made a stele with the top where the bottom ought to be; but if the rock was lying on the ground two engravers might work at it from opposite sides and make steles pointing opposite ways.

2. The script in the new inscription from Mine L, no. 357, is in the main identical with that on the fragments. But it is not by the same hand. If one may employ the language of palaeography, it is probably later and certainly somewhat more standardized. For instance, the fish which represents 'samekh' is a detailed sprawling symbol in the fragments, but in the inscription in the mine it is a neat small character, still clearly a fish, but without so much detail. It is therefore probably true that the fragments are earlier, and inasmuch as the position of the new inscriptions (as well as their meaning according to Professor Butin) implies that the new inscription was put up while

the mines were being worked, the fragments must equally antedate the closing of the mines.

3. It seems to us quite incredible that all the inscriptions on Serâbît have been found. We were there only a few hours, with no proper equipment, at a time of year when work in the sun was arduous and scarcely safe. Yet we stumbled upon two new inscriptions and one new fragment, and, as it now appears, failed to see one fragment which almost certainly is there. It is inconceivable that we have found all that exists. We therefore intend to go again at the earliest opportunity, probably in the spring either of 1929 or of 1930, with the proper equipment, and to explore the whole site. We have reason to hope that Professor Butin and Dr. Gardiner will come with us.

Finally we wish to express our warmest thanks to all the officials who helped us so generously in Egypt, and especially to Mr. and Mrs. Hoops at Um Bogma, whose help and hospitality are one of the pleasantest memories of our journey.



# THE SERÂBÎT INSCRIPTIONS

## II. THE DECIPHERMENT AND SIGNIFICANCE OF THE INSCRIPTIONS

ROMAIN F. BUTIN, S.M.  
CATHOLIC UNIVERSITY OF AMERICA

### I

#### IDENTIFICATION OF THE SIGNS

Beginning with Flinders Petrie scholars have been struck by the similarity between the script of these inscriptions and the Egyptian hieroglyphs. As the inscriptions were found in the neighborhood of numerous Egyptian monuments, an attempt was naturally made to read them as in the Egyptian language; but this proved unsuccessful, and it was evident that although the signs show close relationship with hieroglyphs, the language is not Egyptian. On Sinai, it was natural to think of a Semitic language. Further, the Egyptian phonetic value of the signs does not yield Semitic words, so that it appears that not only the language but the phonetic value of the signs is Semitic, not Egyptian.

It was long ago advocated by Charles Lenormant<sup>1</sup> and has been accepted by many since his day, that the Hebrew alphabet was formed on the principle of 'acrophony,' whereby the phonetic value of the letter was taken from the initial sound of the Semitic name of the object represented by the letter. The name of each letter has been studied in order to determine its original shape.<sup>2</sup> It has been found that occasionally the South-

<sup>1</sup> See François Lenormant, *Essai sur la Propagation de l'Alphabet Phénicien*, I, p. 87.

<sup>2</sup> Nöldeke, *Beiträge zur semitischen Sprachwissenschaft*, 124-136; Lidzbarski, 'Die Namen der Alphabetbuchstaben,' *Ephemeris*, II, 125 ff.; Sayce, 'The Origin of the Semitic Alphabet,' *PSBA*, 31, pp. 215 ff.; Gardiner, *op. cit.*; Grimme, *Althebräische Inschriften vom Sinai*, 25 ff. Sethe has gone over the field again in a series of articles which deserve all praise: 'Der Ursprung des Alphabets,' in *Nachrichten*, Göttingen Academy, *Geschäftliche Mitteilungen*, 1916, 88-161; 'Die Neuentdeckte Sinai-Schrift und die Entstehung der semitischen Schrift,' *ibid.*, 1917, 437-475; 'Die wissenschaftliche Bedeutung der Petrie'schen Sinaifunde,' *ZDMG*, 80, 24 ff. The older works of

ern Semitic group has names different from those of the Phoenician-Aramaic group, and that the Greeks, who borrowed the alphabet with its Semitic names from the Phoenicians, have not always used exactly the same term found in Hebrew. The result of this investigation has been to show that, with slight changes, the present Hebrew names seem to be substantially correct. There is really a striking correspondence between the objects pictured in our inscriptions and the Semitic names as preserved in the Hebrew alphabet; it is also a fact that the phonetic value of the sign is clearly the value of the first letter of its name.<sup>3</sup> This does not necessarily mean that the objects and their pictorial representation as we have them were the only signs used during the formative period of the Semitic alphabet. Other objects, and consequently other signs with the same acrophonic value, may also have been employed; if so, they have been gradually discarded, although some of them may still lie at the basis of certain actual divergencies between the various Semitic alphabets.<sup>4</sup>

Praetorius should not be forgotten. More recently Hans Jensen has published his *Geschichte der Schrift*, Hannover, 1925; see the bibliography there given. To forestall misunderstanding of the necessarily controversial discussion of technical questions which follows in the present article, I would emphasize that the criticism by other scholars of Grimme's translation of the inscriptions has given to many an inadequate impression of the value of the rest of his work. His application of genuine scholarship to the problem has shown the way to further progress, and from his publications on the Sinai inscriptions there is much to be learned.

<sup>3</sup> The principle of Lenormant has been rejected by Bauer-Leander, *Historische Grammatik der hebräischen Sprache*, 1922, pp. 62 ff.; but the correctness of the acrophonic principle is emphasized by the new inscriptions. In these, in all cases where we are sure of the meaning of the name, we find that it corresponds accurately to the object designated. There are obscurities yet remaining, but where two thirds of the letters verify the hypothesis and agreement is being reached for the others, the hypothesis passes into moral certitude.

<sup>4</sup> See the remarks of P. Ronzevalle, S. J., 'Note sur le Texte Phénicien de la Flèche publiée par M. P. E. Guigues,' in *Mélanges de l'Université Saint Joseph*, XI, 329 ff.; Lidzbarski, *Theol. Lit.-Zeitung*, 1921, p. 50. Arguing on this principle, some scholars have proposed for some of the Sinaitic signs a phonetic value which has now been abandoned. Thus the fish of our Sinaitic inscriptions (samekh) was given the value of dalet, first letter of Hebrew דָּ 'fish'; the sign which we read as shin was read as qoph, because it was thought to be a bow, Hebrew קֶשֶׁף. See Cowley, 'The Origin of the Semitic Alphabet,' in *JEA*, III, 17 ff. The services of Cowley in the decipherment of the Sinai inscriptions should have received more recognition than has generally been given them.



Not only have scholars tried to determine the phonetic value of the Sinaitic signs by means of the Semitic names given to the letters in the various groups, but also by the form of these letters in alphabets derived from them directly or indirectly. In recent years Gardiner, Sethe, Grimme, and Ullman<sup>5</sup> have done good work in this respect. Ullman has compared them with ancient Greek, and Grimme has called particular attention to what he calls Old Thamudean.<sup>6</sup>

The relative agreement among scholars as to the respective phonetic value of the Sinai script is very satisfactory. In the regular alphabet, based on the number of letters in the Northern Group, there is disagreement only with regard to two or three signs.<sup>7</sup>

A special difficulty resides in the fact that there are more signs in the inscriptions than there are letters in the Semitic alphabets. Sethe had already counted thirty-two signs, and the newly discovered inscriptions add six more. This problem is connected with that of the protosemitic alphabet and its consonantal system. From a comparison of the various Semitic alphabets it results that in some of the later alphabets sounds originally, or at least formerly, distinct, have been merged into one. There must have been two sounds represented now by

<sup>5</sup> Gardiner, Sethe, and Grimme in the writings cited above; also Grimme in his *Die Lösung d. Sinaischriftprobleme: die althamudische Schrift*; Ullman, 'The Origin and Development of the Alphabet,' in *American Journal of Archaeology*, 1927, pp. 311 ff.

<sup>6</sup> It is very questionable, however, whether there is any justification for distinguishing in the Thamudean script two successive stages, one ancient and the other recent. As Savignac points out (*Revue Biblique*, 1927, p. 275 ff.), there is not a single inscription written in its entirety in the so-called Old Thamudean. The Old Thamudean alphabet has been elaborated by selecting signs from a number of inscriptions in which they occur along with other signs classified as New Thamudean. It is also questionable whether any of the Thamudean inscriptions are really ancient. PP. Jaussen and Savignac are of opinion that the Thamudean inscriptions are later than the Mineo-Sabean or than the Lihyanitic. The Midianite inscriptions on the eastern shore of the Gulf of Aqabah have never been collected. It is probable that the Semites of our inscriptions came in contact with the Midianites, as we read at a later date in the history of Moses, and it is greatly to be hoped that an expedition can soon be organized. The two Dominican scholars have often planned one, but hitherto political circumstances have prevented it.

<sup>7</sup> See below, the Remarks on the Sinai Alphabet. Waw, yod, zain are some of these controverted signs.

the Hebrew heth, *h* and *h*; zain stands not only for ancient *z*, but also for *d*; sade now represents what in the primitive language must have been three different sounds: *s*, *t*, and *d*. There were also two sounds of *t*: *t* and *t*.<sup>8</sup>

But were those extra sounds represented by special signs? This is by no means certain. Many of the letters found in the Southern group and lacking in the Northern group seem to be mere modifications of other signs and not an independent creation.<sup>9</sup> At the beginning one sign apparently did duty for two or more related sounds, and the correct pronunciation depended on the knowledge of the language, very much as with us the correct pronunciation of *th* is based not on a diversity of sign but on familiarity with the words (by itself, it may be *t* or *d*). In our inscriptions it is probable that *h* and one variety of heth are not differentiated. In almost all cases, we have found that the signs of the Serâbit alphabet are reducible to those of the Phoenician group. On the other hand, the names of the Arabic or Ethiopic letters not found in Hebrew are evidently mere suggestions of the sound and have no special meaning; hence not only the sign but also the name seems to be of more recent origin, at a time when a desire was felt for differentiation in script as well as in pronunciation. All told, we think it far more probable that the Northern or Phoenician alphabet was the more primitive, and was the one that probably existed at the time of the Serâbit inscriptions, although it also seems probable that at that time there were more sounds in the language than we find now.

From the inscriptions it is certain that the same letter could be represented by slightly different signs. At that stage of the alphabet the signs were not yet rigidly conventionalized, nor the details rigorously determined; as long as the picture fulfilled its purpose by identifying the object, it had done its duty. Thus we have many variations of the same letter, as in

<sup>8</sup> See Bergsträsser, *Hebräische Grammatik*, p. 86 ff.; on 'and *g*, see p. 37; see also the bibliography given by the author. When speaking of *t* and *t*, we do not refer to the double sounds of the Beḡadkepat; these are not different letters, but only different pronunciations of the same letter according to its relation to a preceding vowel; see E. A. Speiser, 'The Pronunciation of Hebrew,' *JQR*, 1926, pp. 370 ff.

<sup>9</sup> Sethe, *Neuentdeckte Sinai-Schrift*, pp. 459 f.



the case of aleph, beth, lamed, 'ayin, mem, and others. The precise form would be determined according to the space available, the material condition of the slab, the skill of the engraver, and partly also by the preference of the writer. Perhaps this obvious absence of a strict convention is responsible for the divergencies that exist between the Northern and Southern alphabets. Waw may be taken as an example. It can be assumed that then as now there were several kinds of hooks and supports; a hook with a round knob and the ordinary form are both 'hooks,' and both are waws. Even the same object would vary in shape when viewed from different angles: the round-headed hanger, when viewed from the front, would be a mere circle with possibly a bar through it to indicate the stem; if viewed from the side the shape would be very different, yet both would be waw.<sup>10</sup>

Grimme has suggested the existence of combined letters or ligatures.<sup>11</sup> This I have not been able to verify in the inscriptions themselves, and it seems that no such method could have been resorted to at the time of the Serâbit inscriptions. A combination of signs such as is found for instance on the Roman consular coins, and as is proposed here, implies a long familiarity with the signs as independent units. If it should ever be proved that such combinations exist at Serâbit, we should have to assume that the origin of the alphabet is much earlier than our inscriptions. In but one sign in the Serâbit inscriptions could a ligature be suspected, namely our sign no. 27. There we find two letters together, but this is only the result of an accident and does not prove that the engraver knew of the method of ligatures, still less that he used it designedly. All such apparent combinations of letters can be better explained by accident or by an attempt at correction, of which epigraphy furnishes many examples.<sup>12</sup>

The real test of all hypotheses about the values of the signs will be found in their application in the inscriptions themselves.

<sup>10</sup> Sethe, *Ursprung des Alphabets*, p. 134.

<sup>11</sup> *Althebräische Inschriften vom Sinai*, p. 32. In this he was followed by Furlani in at least one instance; see the review of Grimme's work in *Rivista degli Studi Orientali*, X, 693 ff.

<sup>12</sup> Lidzbarski, *Handbuch der Nordsemitischen Epigraphik*, pp. 128 f.

If the value assigned to a sign yields consistently good results, in keeping with the genius of the Semitic languages and with the historical and psychological circumstances under which the inscriptions were written, there is good reason to believe that value to be correct. If on the contrary the sense obtained does violence to well-established lexicographical or grammatical rules, or seems far-fetched and fanciful, then we should be dissatisfied with the phonetic value attributed to the sign.

A table of the signs found in the inscriptions of Serâbît, together with other alphabets for purposes of comparison — Phoenician (Aḥiram and Mesha), Thamudean, Mineo-Sabean, and Ancient Greek is given in connection with this article (opposite page 1). I add to the table remarks on the signs and their phonetic value.

### *Remarks on the Sinai Alphabet*

The Thamudean Alphabet is taken from Hubert Grimme, *Die Lösung des Sinaischriftproblems*, p. 47; the Mineo-Sabean from Guidi, *Summarium Grammaticae Arabicae Meridionalis* (reprint from Le Muséon, XXXIX); the Early Greek from Ullman, 'The Origin and Development of the Alphabet' in *American Journal of Archaeology*, 1927, pp. 311 ff. The alphabet of the Aḥiram inscription has been published in all the leading reviews.

1. Aleph. There is complete agreement among scholars; the ox-head is not yet conventionalized but is clear in all the forms.

2. Beth. There is also complete agreement. The form given by Sethe under his no. 30 does not actually occur. It is given by Gardiner in Inscription 350, but the little square at one of the corners is evidently due to weathering. Ullman includes under beth our sign no. 23 on the strength of the subsequent Phoenician form of that letter, but I have no doubt that it is a resh (human head) badly formed. Beth occurs three times in Inscription 352, and always with its usual form; it is not likely that the engraver would have given such a different form in the same inscription.

3. Gimel. There is also agreement. Wonder has been expressed that in the inscriptions already known (nos. 345-355)



the curved form of the boomerang had not been found instead of the right angle formed by the two lines; Inscriptions 357 and 358 supply the desired form.

4. Daleth. This letter does not seem to occur in any of the inscriptions that the present writer has examined, but it occurs in 345.

5. He. Sethe has conclusively shown ('Neuentdeckte,' p. 444) that this letter is derived from the posture of a man praying with uplifted arms. One form of heth is the same as the he, and it is always doubtful, except from the context, whether we should read he or heth. See no. 26.

6. Waw. The meaning of the word is 'hook,' 'support.' The form given is found four times in the inscriptions. Grimme, on the strength of the Southern group, gives a circle or "rosette," as the Sinaitic waw. I have not found it in the inscriptions which I have examined and there is no doubt that the form given is really that of the inscriptions. See however the remarks made above on the possibility of different pictorial representations for the same object.

7. Zain. This sign consists of two parallel lines, generally horizontal but once (356) apparently vertical. The lines are not connected, except once and that doubtfully. The form given by Grimme of two lines not parallel is found only once, and then is probably the result of lack of skill on the part of the engraver.

8. Heth. As remarked under he, this letter may have the same sign as he itself. There is almost certainly another sign for heth which occurs two or three times and which I have included in the alphabet; it is the sign for one of the heth sounds in Egyptian which might have been borrowed directly, both sign and sound, all the more easily perhaps because in Egyptian it is one of the "cords and plaited-work signs" and 'cord' in Hebrew is חבל, which would give the same sound according to the principle of acrophony. But this is only a supposition. This form may lie at the basis of the Phoenician heth.

9. Teth. Sethe ('Ursprung,' p. 94, note 2 and p. 102, note 2) claims that teth is a modification of taw by the addition of a circle. The name 'teth' has no special meaning according to

him. The form that he proposes is correct, but whether the explanation is right or not it is probably too early yet to decide.

10. Yod. Gardiner, Sethe, and most scholars see the hand as the sign for yod. It occurs in 349. In a modified form it is supposed to occur also in 345. I have included it in curved brackets but consider it more probable that this form is a modification of kaph. A further variation is possibly but not probably to be found in sign 26. Grimme has given a different sign for the yod in the Sinaitic inscriptions but I have not found it. It should be remembered that the scriptio plena is used very seldom, if ever, in our inscriptions, and the *matres lectionis* cannot be expected to appear often. Thus he, waw, and yod are rare.

11. Kaph. Whatever may have been the meaning attributed to the word at the beginning, Sethe and Grimme are probably right in deriving the sign from a plant-sign in Egyptian. If so, sign 25 would be the older as against the one given under 11. Essentially it consists of three shoots with a line underneath to indicate the ground. One of the side-shoots combines with the ground to give us sign 11. In Old Phoenician, the ground-line was left out and we have the three prongs connected at the base. Read as a kaph those forms give a fairly good meaning, and there is no good reason to look for any other phonetic value. Ullman, I believe, is wrong in exchanging signs 10 and 11 and reading kaph under the former and yod under the latter. So far as the names are concerned, the change would be possible, but a yod read in passages where sign 11 or sign 25 occurs would yield no sense.

12. Lamed. There is perfect agreement on the value of the sign, although there is disagreement as to the meaning of the name. It will be noticed here also that the direction of the sign is arbitrary and that it has not yet been conventionalized.

13. Mem. All are agreed on the sign and its phonetic value. The forms are only slightly different, enough however to show that the sign was not strictly conventionalized.

14. Nun. Originally probably *naḥas*. The sign is that of the serpent in its original meaning of a creeping thing. Any form that represents the serpent sufficiently is supposed to



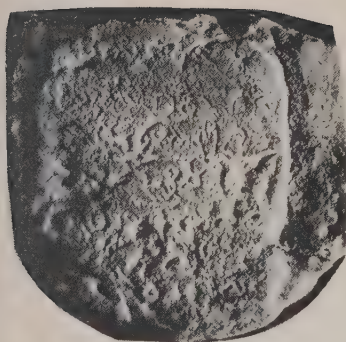


(a) Serâbît, face of Mine L, showing the inscriptions in situ, to the left of the thorn-bush



(b) Serâbît, looking down from the scarp, showing the inscriptions in situ





(a) Inscription 349



(b) Inscription 350



(c) Inscription 352







Inscription 351







Inscription 353



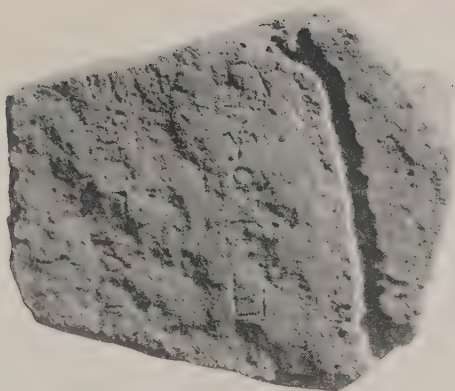


Inscription 354

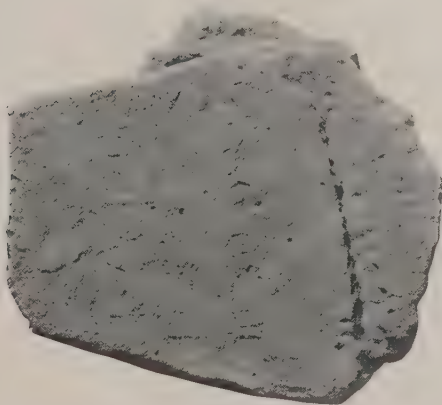




(a)



(b)



Inscription 356, two views with different lighting

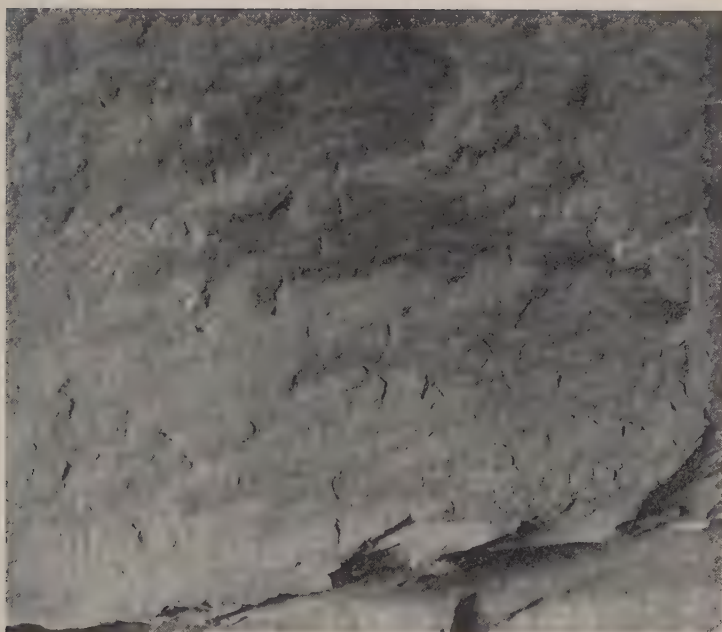






Inscription 357, showing the vertical column complete





(a) Inscription 357, showing the horizontal line complete



(b) Inscription 345  
front view



(c) Inscription 345  
side view





fulfill its function; hence the various modifications that we find in the inscriptions.

15. Samekh. The fish. Gardiner had taken both the serpent and the fish for a representation of nun. Sethe rightly differentiated the two. The word is still used in the Southern group for 'fish.' Those two letters occur so often that no doubt remains as to their value.

16. 'Ayin. The word means 'eye,' and it is under that form that we find it in the inscriptions; sometimes the eye is given with the pupil, at other times without it; once it appears with the eyelids. The eye is generally horizontal, but if space or some other reason requires, it is given vertically. On this point also there is agreement.

17. Pe. The word means 'mouth.' Its form is apparently a small square, smaller than beth and thus distinguished from it. Although it probably occurred several times in the inscriptions, we have found it but once clearly engraved. If it existed elsewhere, it has been so weathered as to be unrecognizable.

18. Šade. Gardiner did not identify this letter. Sethe gives it as a modification of shin, having one of the loops completely closed by a circle, in the same way as teth would be a modification of taw (see above, 9). He sees the sign in Inscription 349, where that sign was given by Gardiner. But the sign does not really occur and we have an ordinary shin, the confusion being due to an evident break. The sign for šade is the one given by Eisler and Grimme, consisting of two circles, one larger than the other. As it occurs several times and consistently yields good sense, we may feel pretty certain that it is correct. A sign transcribed by Gardiner in 349 and interpreted as aleph by Sethe and Eisler is really a šade. The letter is both horizontal and vertical. But the meaning of the name still remains obscure.

19. Qoph. The sign for this letter is suggested by Sethe ('Neuentdeckte,' p. 448) and accepted by Eisler ('Weihinschriften,' p. 100), Grimme, and Ullman. It does not exist in the Aḥiram inscription, but Greek has preserved the sign accurately and the qoph of the Mesha stone is very close to it, as is the case also in the Southern Semitic alphabets.

20. Resh. The sign for resh is certainly the human head, and on this point there is no disagreement among scholars. A certain freedom of treatment is evident, but as long as the human head could be recognized the details were considered secondary.

21. Shin. This letter occurs very often in the inscriptions, and there is no doubt about the value of the sign, on which the agreement is complete. The form of the letter is also very consistent throughout.

22. Taw. This is also a common letter in the inscriptions, and since the sign has been preserved in all Semitic languages with the value of a taw, there is almost perfect certainty with regard to it.

23. The sign given under 23 is not mentioned by Sethe; Eisler (p. 96) sees in it a variation of lamed; Grimme ('Sinai,' p. 32) takes it as a combination sign for 'ayin-lamed; Bruston (*Revue Archéologique*, 1921, p. 59) reads it as a variation of resh. After examining the original, I feel confident that a resh, facing left, is intended. At any rate, as I have already noted, it is not likely that ligatures should occur at that early date.

24. This occurs in 356 and possibly in 352; in the latter case, I am inclined to see the same sign as no. 25, for there is apparently a base and the two side-shoots spring from the central one but a short distance up from that base. The sign however certainly occurs in 356. However, in 352, Sethe, Eisler, Bruston, and Grimme have all read the sign. Eisler understands it as zain; Grimme as yod; while Sethe and after him Bruston read a variation of taw. Sethe is probably right, as usual. Apart from the shape itself, which is suggestive of taw, the sign understood as taw yields a good sense in 356, although in 352 it apparently forms a proper name with sign no. 23, and it remains doubtful whether we should read T-R or K-R.

25. We have already spoken of this sign under 11. It occurs in 350 and 352 (twice, if I am right in my suggestion under 24). In 352 the phonetic value of kaph yields good sense, but that is not so clear in 350, although, as we shall see, it is possible.



26. This is a new sign, from Inscription 357, where it occurs as the last letter of the inscription. It is roughly a triangle with two horns on the sides at different heights. It is to be noted that the lines of the triangle do not touch. For lack of space the letter has evidently been contracted and thereby distorted. It may be a yod, hand with three fingers, or aleph, the triangle being the head and the hooks forming the horns; or more probably a he, the arms not being extended farther up for lack of space.

27. I feel certain that this sign, though not a combination, stands for two signs written too closely together, lamed and aleph. I hesitated to put it in the alphabet, but finally decided to do so in order to give an opportunity for controlling my reading.

28. The sign hardly shows on the photograph, but Professor Blake and his colleagues, who saw the inscription, have no doubt as to its main lines. Some chisel-marks inside the sign toward the top make me suspect that we may have another form of lamed, with some weathering on top. If the sign is really as it is represented, the letter to which it could be most nearly assimilated would be kaph. But this is very problematical.

29. This seems to consist really of two signs, the second being 'ayin, while the first is not complete and has probably been injured by a break. But it is hard to know what it really is.

30. No photograph could be taken of Inscription 358 in which the last three new signs occur; but I have no doubt of the accuracy of the transcription. The sign might be a pe, were it not for the fact that pe, with a different shape, occurs in that same short inscription. For the same reason it can hardly be a beth. The letter seems to belong to a proper name, and no clue can be had from the sense.

31. I suspect that this sign is a dalet, the line of the panel having been omitted, but the matter is doubtful.

32. It is almost certain that this is really a variation of 'ayin, the eye being here represented with the eyelids.

It is highly probable that in the course of time all the remaining doubts about the signs will be cleared up. It may be added

that Jensen in his great work, *Geschichte der Schrift*, 1925, pp. 109 ff., gives a good historical sketch, but has hardly any criticism or discussion of the values of the signs hitherto proposed. He approves in general of Sethe's identifications, but holds that Sethe has gone astray in the case of a few letters, yod, waw, šade, qoph, as well as on the differentiation between he and heth. But we have seen that Sethe was probably right.

## II

### DATE OF THE INSCRIPTIONS

Flinders Petrie ('*Résearches in Sinai*,' p. 61) is of opinion that the inscriptions cannot be later than the reign of Thutmos III, nor can they be much earlier, for Mine L exhibits some traces, for instance pottery, of the reign of that pharaoh. Moreover, no. 346 was found at the door of the shrine of Sopdu, which, Petrie says, was built by Queen Hatshepsut near the Temple of Hathor, and the sphinx, no. 345, is of red sandstone, which was used by Thutmos but not at other times. Also, this small sphinx had apparently the Horus-name of Snefru inscribed between the paws, and he is called "the beloved of Hathor, Lady of Turquoise." Snefru was venerated particularly by Queen Hatshepsut, and the date is inferred to be the period of the famous queen. All these reasons put together form a cumulative argument which induces Petrie to assign the inscriptions to the reign of Hatshepsut and Thutmos III, or roughly speaking to 1500 B.C.<sup>13</sup>

However, both Gardiner and Sethe are inclined to assign an earlier date to those inscriptions, and are probably right.<sup>14</sup> Queen Hatshepsut did not build the shrine of Sopdu but rather restored and embellished it; it was erected during the XIIth dynasty.<sup>15</sup> The fact that red sandstone was used for the sphinx is not conclusive in dating it in the reign of Thutmos III, for it does not seem to be a pure Egyptian monument; all the inscriptions, including the statuette, no. 346, are of the same

<sup>13</sup> Flinders Petrie, *Researches*, pp. 129-131.

<sup>14</sup> Gardiner, p. 13; Sethe, *Neuentdeckte Sinai-Schrift*, pp. 465 ff.; *Wissenschaftliche Bedeutung*, p. 40.

<sup>15</sup> *Neuentdeckte Sinai-Schrift*, p. 465.

material, the most common at Serâbît. Queen Hatshepsut was undoubtedly a great devotee of Snefru, but the veneration of this pharaoh did not begin then; in fact it was during the XIIth dynasty that he was deified and worshipped as the founder of the mines of Serâbît. Again, it is far from certain that Mine L, near which most of the inscriptions have been found, was worked exclusively by Thutmos III; pottery of a later date could easily be placed in an older mine, especially when we remember that Mines L and M were the first met on the way up to the temple of Serâbît.<sup>16</sup>

The conclusion of Flinders Petrie that the inscriptions should be assigned to about 1500 B.C., while possible in itself, is not supported by any conclusive argument. On the other hand, there are stronger reasons for assigning them to an earlier date. Sethe<sup>17</sup> has called attention to the vertical direction given in our inscriptions to some letters which normally should be horizontal; this practice was a peculiarity of the Middle Kingdom (dynasties XI–XIV), and does not exist during the New Kingdom, which is the period to which Petrie would assign our inscriptions.

Gardiner, and after him Sethe, also refers to the treatment of Ptaḥ in Inscription 351. It is the treatment common during the XIIth dynasty, but is not to be found after that period.

Since Flinders Petrie wrote his 'Researches,' another document has come to light at Byblos, the Aḥiram Inscription, dating probably from the thirteenth century B.C.<sup>18</sup> This inscription contains all the letters of the Phoenician alphabet except three, daleth, ṣade, and qoph. Two of the missing letters, daleth and ṣade, are supplied by a contemporary inscribed arrow-head found by P. E. Guigues at Roueisseh near Nabatyeh in Southern Syria,<sup>19</sup> so that we have practically the complete

<sup>16</sup> Flinders Petrie, *Researches*, pp. 62 ff.; Gardiner and Peet, *Inscriptions of Sinai*, Plate LXXXIV.

<sup>17</sup> *Neuentdeckte Sinai-Schrift*, p. 466.

<sup>18</sup> See, however, Spiegelberg, *Orientalische Literaturzeitung*, 1926, 735 ff. On the Aḥiram inscription, see among others H. Vincent, *Revue Biblique*, 1925, and the bibliography there given.

<sup>19</sup> See Guigues, 'Pointe de flèche en bronze,' in *Mélanges de l'Université Saint Joseph*, XI, 325; Ronzevalle, 'Note sur le texte phénicien de la flèche publiée par M. P. E. Guigues,' *ibid.*, 329 ff.

alphabet of the thirteenth century. This alphabet shows a decided advance on that of our Sinai inscriptions; the pictorial character has almost completely disappeared and the letters are greatly conventionalized. We may well ask whether two hundred years are sufficient to account for the development of the script. The differences between the two series are certainly much greater than those existing between the Aḥiram inscription and that of Siloam, although the interval between these two is more than 500 years. If the rate of change and modification was the same, which of course it is not necessary to assume, we should need for the passage from the Sinai inscriptions to the Aḥiram inscription a much longer time than that which lies between the Aḥiram and Siloam inscriptions.<sup>20</sup> Surely this consideration would make the XIIth dynasty a much more probable date for the inscriptions than the XVIIIth.

I am therefore convinced that our Sinai inscriptions are older than 1500 B.C. But how much older? The evidence thus far adduced points to the XIIth dynasty, or roughly to about 1900 B.C. Sethe thinks that they should be put a little later, in the Hyksos period. The Hyksos were Asiatics, probably Semites, and came into contact with Egyptian methods and culture; they would naturally try to do for their language what the Egyptians had done for theirs, and so may have elaborated the alphabet which we find at Serâbit. Sethe insists on the fact that the sphinx, no. 345, bears an inscription in both Egyptian and Semitic; between the paws is the Horus-name of some pharaoh, with the words, "Beloved of Hathor, the Lady of Turquoise," as predicate to that name. This Horus-name does not belong to any of the kings of the XIIth dynasty, which are all known. So, continues Sethe, it can belong only to one of the obscure kings of the XIIIth or XIVth dynasty, which were Egyptian, or better still to the Hyksos. The Semitic inscription is almost a word-for-word translation of the Egyptian; why should a purely Egyptian king feel the

<sup>20</sup> If von Bissing had known the inscription of Aḥiram, he could not have assigned such a relatively late date to our inscriptions as he does in his *Die Datierung der Petrie'schen Sinaiinschriften*, 1920; see the criticism by Sethe, *Wissenschaftliche Bedeutung*, p. 40.



need of having a dedication translated into a Semitic language? Accordingly the sphinx belongs to the Hyksos period, and if the sphinx, then all the other inscriptions, which are evidently of the same date.<sup>21</sup>

Sethe assumes that the Horus-name in question is that of the pharaoh who dedicated the sphinx. This is by no means certain, and we have seen that Flinders Petrie reads the name of the deified Snefru not as the giver but as the recipient of the gift. If so, the sphinx could have been inscribed by any devotee or devotees of Snefru. We do not know the reason why we have a bilingual inscription in the temple of Hathor, but at least it seems certain that it was placed there by a Semite, or by one who would cater to the Semites. The assertion that the Egyptians would not have allowed any votive object to be placed in the Temple of Hathor with a Semitic inscription remains to be proved; the goddess was a Semitic goddess, just as was the Lady of Byblos, and while the Egyptians would naturally use Egyptian in their dedications, no reason is apparent why a Semite could not do the same in his own language; further, we might reverse the supposition and ask: if a Semite (Hyksos) dedicated that sphinx to a Semitic goddess on Semitic soil, why should he put his inscription in Egyptian at all? But all this is but speculation, and we do not possess positive data for a decisive answer. As far as we know, the Hyksos kings did not work the mines of Sinai, and mining was resumed only under the XVIIIth dynasty. Nor do we find any authority for the assertion that a private enterprise by the local Semites of Sinai was at some time set on foot, an idea which would be contrary to what we know of the people of the Peninsula. Sethe seems throughout to assume that our inscriptions are the beginning of the Sinai alphabet; there are, as we shall see, reasons for thinking that this was not the case.

It is evident from our inscriptions that at the time when the inscriptions were written the Semites formed a well organized body of workers under various officials. The names of some of

<sup>21</sup> See *Neuentdeckte Sinai-Schrift*, pp. 465 ff.; *Wissenschaftliche Bedeutung*, pp. 40 ff.

them are mentioned in the inscriptions, particularly a certain M-SH. On the Egyptian monuments we find an organized band of Semites only under the XIIth dynasty, and more particularly in the reign of Amenemhat III. They are the Retenu and 'A'amu.<sup>22</sup> These Semites were not Beduins living on Sinai but, as is almost universally admitted, Phoenicians from Syria and Palestine. That they are never mentioned after the reign of Amenemhat III probably implies they were not there, at least as an organization, after that date. In the XIIIth and XIVth dynasties the mines were not worked intensively and probably extra workmen were not needed; there is no record of work during the Hyksos period and probably there was none. Under the XVIIIth dynasty we may assume that Semites were not wanted on account of the feelings of both Egyptians and Semites immediately after the expulsion of the Hyksos by that dynasty. This leads to the XIIth dynasty and the reign of Amenemhat III as the most likely date for our inscriptions.

In Inscription 357, if our reading is correct, we find mention of a certain R-M and עמה 'his people' or 'subordinates.' The term עם may also mean 'people' or 'nation,' hence 'compatriots.' By reading עמה as a plural, עמיה = עמיו, we may translate 'his kinsmen' or 'members of his clan.' If this latter rendering is correct, we should have evidence of a clan of Semites under their leader R-M taking work at Serâbit just as we know the Retenu and the 'A'amu did under Amenemhat III. Perhaps we may even see in the term a counterpart of the name 'A'amu given these by the Egyptians. At any rate this would agree well with what we know of conditions under Amenemhat III.

In the same Inscription 357 there is probably a mention of a gang of miners consisting of nine men. They were evidently Semites, since the record is written in Semitic. The organiza-

<sup>22</sup> Whether the word 'A'amu is derived from the Semitic אָמ, as is commonly believed, or is a genuine Egyptian word from אָמ 'boomerang,' as maintained by W. Max Müller, *Asien und Europa nach altägyptischen Denkmälern*, pp. 121 ff., it is apparently certain that the word was pronounced 'A'ami, plur. 'A'amiw, as is attested by Coptic 'ame,' plur. 'amiou' 'herdsman.' In either case, it must have been a gentilicium, 'member of the people' or 'boomerang-thrower.' On the presence of the Semites, see nos. 24, 85, 110, 112, in Gardiner and Peet, *Inscriptions in Sinai*.

tion of miners into gangs also corresponds to the period of Amenemhat III, although no doubt it was also practised at other times. The important thing to remember in this connection is the solidarity of the Semites at Serâbît; as a result of the success of that gang, R-M and his 'subordinates' (or possibly 'compatriots' or even 'kinsmen,' 'clansmen') celebrated a feast; in the success of the gang was the pride of the workmen. The Egyptians apparently did not object, a circumstance which would show friendly coöperation; and this again fits nicely with the conditions under Amenemhat III. What was the office of R-M is problematical. He may have been the 'Sa,' foreman of a gang of miners, or 'Mer Sa,' head of a section of foremen;<sup>23</sup> but he may equally well have been the leader of a clan of Semites, 'A'amu or Retenu. When we add to this what was said above of the Ahiram inscription, the conclusion follows that while recognizing the possibility of Sethe's contention, all the evidence is in favor of the XIIth dynasty, and more specifically of the reign of Amenemhat III, as the period of our Sinai inscriptions. This result agrees with that of Gardiner, and until more evidence comes to light a date for the inscriptions (not, of course, for the origin of the alphabet) at about the year 1850 B.C., or a little later, seems probable.<sup>24</sup>

### III

#### ORIGIN OF THE SINAI ALPHABET

It is impossible here to discuss in detail the origin of the Sinai alphabet, a study which relates also to the origin of the Phoenician alphabet. Suffice it to say that connection of both these with the Egyptian hieroglyphs must be maintained against all other theories.<sup>25</sup> The arguments of Sethe seem en-

<sup>23</sup> See Flinders Petrie, *Researches*, pp. 116 ff.; and no. 85 in Gardiner and Peet's *Inscriptions of Sinai*.

<sup>24</sup> See Ullman (cited above, note 5), pp. 324 ff. Ullman reaches the same conclusion as the one advocated here; he also maintains that the Sinai inscriptions are not the first attempt at writing in this script. His interesting article presents other considerations which tend to show that our alphabet is older than 1500 B.C. This section of Ullman's article deserves particular attention.

<sup>25</sup> The bibliography on this point is too extensive to be given here. Of the scholars who have written on this problem since the publication of the inscriptions, nearly all

tirely cogent.<sup>26</sup> Egyptian had a real alphabet consisting of 24 (later 26) letters obtained from short monosyllables. It is true that alphabetic signs are not used exclusively, and that the language includes many ideograms and biliteral phonograms, but of all the nations with which the early Semites came in contact the Egyptians are the only one having an alphabet in the strict sense. The writing of our inscriptions is evidently alphabetic, as is shown among other things by the relatively small number of signs.

The Semitic alphabet, as explained above, is based on the principle of acrophony. It is not proved, it is true, that the early Egyptians elaborated their alphabet consciously in accordance with that principle; in fact they probably did not do so. They seem to have used words with only one full fundamental consonant in the same way that they used the biliteral phonograms without any thought of their acrophonic value.<sup>27</sup> Apparently they became conscious of the principle during the Middle Empire under the influence of the so-called enigmatic writing (abbreviation).<sup>28</sup> At the time of the discovery of the Semitic alphabet the acrophonic value of at least some of the letters was well known. Hence there was a method that could be copied, extended, and adapted to the needs of the Semitic language. This model does not seem to have existed anywhere else.

The Egyptians wrote without vowels, a practice which is also a characteristic of Semitic languages, except Babylonian. The other scripts are generally syllabic, and it would seem a step backward to discard vowels if they had been found in the model

have already been mentioned, Gardiner, Sethe, Cowley, Sayce, Eisler, Bruston, Grimme. We may add here Schaefer, 'Die Vokallösigkeit des phönizischen Alphabets,' in *Zeitschrift für Aegyptische Sprache*, 1916; Lehmann-Haupt, 'Zur Herkunft des Alphabets,' *ZDMG*, 73, pp. 51 ff.; Kalinka, 'Der Ursprung der Buchstabenschrift,' in *Klio*, XVI, pp. 302 ff.; Cook, in *Cambridge Ancient History*, III, pp. 416 ff., 739; Jensen, *Geschichte der Schrift*, pp. 109 ff. Jensen still has doubts about the Egyptian origin of the Semitic alphabet; he does not seem to have been acquainted at the time of writing with the latest discoveries at Byblos.

<sup>26</sup> *Ursprung des Alphabets*, pp. 126 ff.; *Neuentdeckte Sinai-Schrift*, pp. 455 ff.; *Wissenschaftliche Bedeutung*, pp. 31 ff.

<sup>27</sup> Sethe, *Ursprung des Alphabets*, pp. 118 ff., 151 ff.

<sup>28</sup> *Idem*, *Neuentdeckte Sinai-Schrift*, pp. 472 f.



from which our Semitic alphabet was derived. Accordingly the vowelless character of the Semitic alphabet points to an original without vowels, which as a matter of fact can only be Egyptian.<sup>29</sup>

The arrangement of the inscriptions also points to Egyptian methods. They are written in vertical columns arranged from right to left, an ancient Egyptian practice; one (349) runs horizontally from right to left, as became customary in Egypt toward 1900 B.C.; when necessity requires, some words run from left to right, a method also found in Egyptian.<sup>30</sup> In fact, at the time when our inscriptions were written and for a long period before, the Semites of Phoenicia and of Sinai had been in constant communication with Egypt. Under the XIIth dynasty they shared in the mining expeditions; at Serâbît and Maghareh they had before their eyes numerous Egyptian monuments, were working under Egyptian officials, and saw the Lady of Turquoise worshipped with a Semitic ritual even by the Egyptians.<sup>31</sup> The whole atmosphere there was Egyptian. If with Sethe we make the Hyksos the inventors of the alphabet, the Egyptian influence is still more manifest.

Above all there is the striking similarity of the Sinai Semitic script to the Egyptian hieroglyphs, not in a few isolated cases but regularly. Of course there would necessarily be some points of contact between the various pictorial systems, and similarities may be pointed out between certain signs of our Sinai inscriptions and Cretan, Hittite, and other signs;<sup>32</sup> but these are, or may be, purely accidental. In the case of Egyptian, there is a prototype for almost every one of the signs of the Sinai inscriptions. It should be noted that this constant similarity becomes more striking if we consider the hieroglyphs

<sup>29</sup> Sethe, *Ursprung des Alphabets*, p. 127; *Wissenschaftliche Bedeutung*, pp. 29 ff.

<sup>30</sup> Idem, *Neuentdeckte Sinai-Schrift*, pp. 461 ff. This practice has survived in many of the subsequent Semitic inscriptions, both in the South and North; see Jaussen and Savignac, *Mission Archéologique en Arabie*, vols. I and II, *passim*; Grimme, *Die Lösung des Sinaischriftproblems*, pp. 33 ff.; Lidzbarski, *Handbuch der Nordsemitischen Epigraphik*, p. 125. On all this section see Sethe, *Wissenschaftliche Bedeutung*, pp. 33 ff.

<sup>31</sup> Flinders Petrie, *Researches*, chapter xiii.

<sup>32</sup> See Jensen, *Geschichte der Schrift*, pp. 102 ff.

of Sinai rather than those of Egypt proper. There is hardly a sign in our inscriptions that is not found in the surrounding Egyptian monuments; when the form of the Sinai hieroglyphs differs in detail from the pure Egyptian, it is invariably the Sinai form that is reproduced in our inscriptions. Ullman has found almost all the signs in one Egyptian inscription, no. 53 of the publication of Gardiner and Peet. We can therefore be more precise in our conclusion, and say that the Semitic alphabet of Serâbit and Maghareh is based not on the hieroglyphs of Egypt but on those of Sinai, themselves of course Egyptian but not carved by the best artists, who must have been kept in Egypt proper for the more important monuments. If we think that the Phoenician alphabet was originally derived from the pure Egyptian hieroglyphs, we must also admit that it was modified by the Sinai Semites after the fashion of the Sinai hieroglyphs. It is almost certain that Semitic workers were on Sinai before Amenemhat III, although not organized as we find them under his reign.

But when we speak of the Egyptian origin of the Semitic alphabet, the phrase must not be misunderstood. What took place was not a servile borrowing of the Egyptian alphabet as it was, signs and sounds; but rather an imitation or adaptation; as Sethe says, "Vorbild" not "Urbild."<sup>33</sup> The Semites borrowed the idea of the alphabet, the principle of acrophony (which they further developed), and also most, if not all, of the signs; but they gave the signs a Semitic phonetic value and a Semitic name. They may have created some of the signs themselves; at all events four-fifths of the signs find their prototype in the Sinaitic hieroglyphs. As to the few signs not yet shown to be related to an Egyptian model, it does not follow that the as yet unidentified prototype did not exist. As we have said, our inscriptions are not to be regarded as the beginning of the Semitic alphabet; other inscriptions may yet turn up that will give us intermediate forms.

Against this conclusion it is no objection to say that the Semites, if they borrowed, would have borrowed the entire Egyptian alphabet, and that exclusively, instead of adapting

<sup>33</sup> Neuentdeckte Sinai-Schrift, pp. 454 f.

signs that occur only as syllabic signs or even as ideograms. The Egyptian alphabet in itself did not furnish the necessary material. For instance, the Egyptian eagle, used for aleph, could not represent that letter in Semitic because the Semitic *nesher* or '*azniah*' would not give the desired letter, and on the other hand, both nun and 'ayin were provided for, the one by the serpent and the other by the eye. It was often a case of choosing between several possible objects. In making his selection the Semitic genius who elaborated the alphabet seems to have been guided by some well defined principle which we do not clearly understand at present; he had to go outside the regular Egyptian alphabet to find what he wanted, and chose his signs from the many hieroglyphs which he saw on the Egyptian inscriptions at Maghareh or Serâbît. For this reason we do not consider it necessary to maintain with Sethe<sup>34</sup> that the Semites who worked on Sinai and carved the inscriptions should have come from Egypt, after living there and becoming acquainted with the hieroglyphic system; they may or may not have had such antecedents, but the models they actually used were the hieroglyphs of Sinai rather than those of Egypt proper. It is generally believed that the Retenu and the 'A'amu had come from Syria and Phoenicia to take part in the expeditions; this view does not require that they should have passed through Egypt on the way.

Some may find a serious difficulty in admitting that such an epoch-making innovation could have been introduced in any other milieu than among educated men and in the literary centres of Phoenician culture. It may be so; yet the opposite is likely to be nearer the truth. First of all, the adoption of the alphabet may have taken place at a time when, so far as we know, there were hardly any such literary centres among the Phoenicians; and secondly, if there were such literary centres, it is not likely that the introduction of the simpler alphabet would have succeeded among them. The learned, familiar with the more complex methods of writing already current, would not have felt the need of a new method, even though it was simpler. They would rather have scorned it as useful to the ignorant

<sup>34</sup> Neuentdeckte Sinai-Schrift, p. 467.

but not worthy of their attainments. The Egyptians, for instance, had every means at hand for the simplification of their hieroglyphic system, and yet, even when this simplified alphabet was in use among the Phoenicians, they did not deem it such an improvement as to lead them to change; the new method may have been good enough for the Retenu and the 'A' amu, who were not capable of doing better, but the Egyptians preferred their own system. It is not the educated Chinese who desire a simplification of their ideogrammatic system; and even among ourselves phonetic spelling does not find many supporters in intellectual circles. Traditions are not easily abandoned, especially when they are the means of maintaining the superiority of an intellectual élite. But a new population, with no traditions of their own, desirous to gain for their language what they saw their neighbors enjoying for theirs, on the other hand incapable of mastering easily the intricacies pertaining to both the hieroglyphic and cuneiform systems, would be the one to adopt an easier and simpler method. Unless we are mistaken, the Phoenician alphabet was not intended for the educated, who were familiar with the standard system of Egypt or of Babylonia, nor was it elaborated merely as a progressive step, but it was devised in order to be useful to an inferior class who could not assimilate anything higher. For a long time the Phoenician alphabet does not seem to have been given its proper recognition, and during the Tell-el-Amarna period one of the great standard systems of writing was still in use among the educated.

Who was the genius responsible for its introduction, we do not know; probably some one of the Semitic leaders who wished to help his people; possibly it may have been suggested to him by some of the Egyptian officials with whom the Semites came in contact and under whom they worked. As has just been said, I am inclined to believe that the alphabet originated in Sinai on account of the closer resemblance of the letters to the hieroglyphs of Serâbît, but it is of course possible that after originating elsewhere the signs should have been modified so as to make them harmonize better with the Egyptian monuments of Sinai.



As alternative to the theory that the hieroglyphs were the prototype of the Semitic alphabet, Grimme proposes the hieratics.<sup>35</sup> I do not believe that this view can be maintained. If there is a relation between the name and the form of the letter, as I believe there is, it is clear that the form must have corresponded to the object referred to by the name. In hieratics the pictorial character has at times completely disappeared, and the object intended cannot be identified by its pictorial representation, unless the latter be explained by its hieroglyphic original. If *ṣade*, for instance, represents, as Grimme claims, the two sides of the face, the original sign for the letter must have been a full face, such as is found in the hieroglyphic script but not in the hieratic, in which the face is entirely unrecognizable. The same is true of *resh*, and many other letters. Grimme might be right, if the Semites had borrowed not only form but also name and phonetic value from the Egyptians, as the Greeks did later from the Phoenicians. In that case the Semites could have taken over the language and its alphabet at any period of its development. But this is not the case; the form was borrowed, but to the object was attached its Semitic name and a specifically Semitic phonetic value given to the letter. There must have been a justification for the name in the actual representation of the object.

It is true that the Sinai script already shows traces of simplified and loosely conventionalized lines, at times resembling the hieratics; but this merely proves that the same need for ease of writing has produced the same results; it also shows that the present Sinai script, though in the earliest Semitic alphabet we have, does not give the original Semitic alphabet, and that there must have been an older stage in which the pictorial character was still clearer and stronger. This is evident from a comparison between our alphabet and the hieroglyphs of Sinai, and the assertion remains true even after due allowance is made for lack of skill on the part of the engraver. Lack of skill would account for the poor execution of the picture intended, but not for the modification of the lines themselves.

<sup>35</sup> *Althebräische Inschriften vom Sinai*, pp. 19 ff. He thus revives the system of de Rougé.



Even in its present forms the Sinai script is much closer to the hieroglyphs than to the hieratics.

## IV

## DECIPHERMENT AND INTERPRETATION OF THE SINAI INSCRIPTIONS

The deciphering of the inscriptions is a difficult task, not so much with reference to the value to be given to the signs, about which there is now comparative agreement, as on account of the danger of taking for a letter what is merely a weather-mark. Hitherto most of these inscriptions have been studied from photographs, squeezes, or copies made by untrained students; the originals have been accessible in only a few cases; namely, 345 in the British Museum, 346 in the Museum of Cairo (examined by Furlani in his checking of Grimme's rendering),<sup>36</sup> 347 in the Musée du Cinquantenaire in Brussels. My own experience has shown that photographs are very deceptive for this purpose. They do not differentiate between an incised sign and mere abrasion, and thus may lead to unavoidable mistake. Even the exact lines of an unquestionable engraved letter may be easily obscured, since the result depends greatly on the direction of the light.

Confusion may also arise because the inscriptions are not read in the right direction. For instance, Bruston does not seem to admit the possibility of vertical columns being read vertically, although this is evidently the arrangement in most of the inscriptions. He lays the slabs on their side and then generally reads from right to left, completely reversing the order of the letters; it is surprising that by this process he could find any meaning at all. These small stelae were evidently intended to be placed upright in the direction of the grooves, and to be read as they were, without the reader's being compelled to bend down and read them sideways. Bruston has also assumed the presence of ideograms, and thereby missed some of the signs intended as letters.

<sup>36</sup> 'Di una iscrizione paleoebraica Sinaitica del Museo Egiziano del Cairo,' in *Rivista degli Studi Orientali*, X (1925), pp. 593 ff. See Sethe, *Neuentdeckte Sinai-Schrift*, p. 452.

From the discovery of the inscriptions by Flinders Petrie in 1905 until their publication (without translation) by Gardiner in 1916 they remained practically unknown. By the use of the two photographs of nos. 345 published by Flinders Petrie in his 'Researches' (figs. 138 and 139), a few attempts had been made to read individual letters and words. Ball's rendering is not correct, but he has the credit of having first identified 'ayin and taw.<sup>37</sup> In 1911 Bruston identified another letter (lamed) and read עלת, which he translated by 'holocaust.'<sup>38</sup> Gardiner identified fifteen letters in 1916, but with his usual caution admitted his inability to read anything with certainty except the word בעלת 'lady,' which occurs several times in the inscriptions. Gardiner must have communicated his article to Cowley, for in the same number of the *Journal of Egyptian Archaeology*, Cowley used it and successfully read על נעם רב in 346; תנה in 347; רב in 349; in addition he identified sign 8 as one form of ḥeth.<sup>39</sup> Hans Bauer made an attempt to read some more words,<sup>40</sup> but his method is too theoretical to be of much service. Further identifications of letters were made by Littmann,<sup>41</sup> Lidzbarski,<sup>42</sup> and especially by Sethe.<sup>43</sup>

Only three scholars have attempted to decipher the whole series of inscriptions in special articles or monographs. They are R. Eisler,<sup>44</sup> C. Bruston,<sup>45</sup> and H. Grimme.<sup>46</sup> A contribution

<sup>37</sup> Proceedings Soc. Bibl. Archaeology, XXX, 1908, 243.

<sup>38</sup> Revue de Théologie, Montauban, XX, 177; XXI, 176.

<sup>39</sup> JEA, III, 17. See above.

<sup>40</sup> Zur Entzifferung der neuentdeckten Sinaischrift, 1918.

<sup>41</sup> Internat. Monatschrift, XV, 248 ff.

<sup>42</sup> Theol. Litztg., 1921, 49. It is difficult to do justice to everyone, as the work has been a slow process, in which many have had a share.

<sup>43</sup> 'Die Neuentdeckte Sinai-Schrift,' 1917.

<sup>44</sup> Die Kenitischen Weihinschriften der Hyksoszeit, 1919.

<sup>45</sup> 'Les plus vieilles inscriptions Cananéennes,' in Revue Archéologique, Série V, XIV (1921), 49 ff.

<sup>46</sup> Althebräische Inschriften vom Sinai, 1923. This was severely criticized by Sethe in the article referred to several times already, ZDMG, 80 (1926), pp. 24 ff. To this criticism Grimme made answer in an article in the same Review, 'Hjatsepsu und die Sinaischriftdenkmäler,' ZDMG, 80, 137 ff, with a Nachwort by Sethe, p. 151. In the same year Grimme also published a more lengthy justification: Die Lösung des Sinaischriftproblems, 1926. Further criticism is found in Schaumberger, 'Die angeblichen Inschriften vom Sinai,' in Biblica, VI (1925), 26 ff., 156 ff., and J. M. P. Smith, 'A New Disclosure from Sinai,' in Journal of Religion, 1926, 195 ff.; the main criticism

by Völter covers mainly Inscription 349, the great centre of the storm raised by Grimme's rendering of it; he accepts Grimme's translation, which he enlarges and modifies.<sup>47</sup> The translators were at complete variance as to the contents of the inscriptions, but it was felt that no further progress could be made until the original inscriptions themselves were made available to the scientific world. This has now been effected, and they can thus be studied at the Museum of Cairo. I take this opportunity to express my gratitude to the authorities of that Museum for their courtesy during the week I spent there examining the inscriptions.

In my work, having first ascertained the character of the signs as accurately as possible, I have adopted a division of words and a translation that seems to me more in keeping with the historical and psychological conditions of the Semites at Serâbît, and also with the laws of Phoenician grammar. Without excluding at the start forms not found in Phoenician or in Hebrew, provided they have parallels, I have always preferred forms that actually occur, even though I have had sometimes to give up very tempting hypotheses. I do not offer my translations as final, but I hope that they will contribute their share to the solution of the problem. Now that the inscriptions are within reach, we may look for greater accuracy in reading and consequently for greater agreement among scholars.

is that Grimme read as letters what was not there. Grimme in his answers has hardly answered his critics on this point, and we shall see that they were right. Yet Grimme has added a great deal to our knowledge and his merit should not be minimized.

<sup>47</sup> 'Mose oder Menassae? Neue Beiträge zu den hebräischen Inschriften der mosaischen Zeit vom Sinai,' in *Nieuw Theologisch Tijdschrift*, 1925, pp. 215-244. Völter has also published a monograph in which he draws what proved to be hasty historical inferences from Grimme's translation: *Die althebräischen Inschriften vom Sinai und ihre historische Bedeutung*, Leipzig, 1924. On the whole controversy see Furlani, 'Yahu. Sapdu e una presunta Iscrizione di Mose,' in *Giornale della Società Asiatica Italiana*, New Series, I, pp. 1 ff.

## V

## REMARKS ON THE INSCRIPTIONS

The *matres lectionis* are never written; there is but one apparent exception (356) and there the waw is probably a consonant.

The article is never used.

Letters are occasionally accompanied by a dot; sometimes apparently to indicate the end of a word where there might be danger of running its last letter into the next word, for instance, 346, 357; at other times each letter is thus marked by a dot; at still other times it is hard to know just why the dots have been used with some letters and not with others, as at the end of no. 346. Vertical columns and horizontal lines are almost always separated by a dividing line.

<i>Pronouns.</i>	Personal.	1st pers. אן, 349 (?).
	Suffix.	1st pers., mater lectionis not written, 356, 353. 2nd pers. fem. ך, 353. 3rd pers. sing. masc. ה, 357 (?).
	Demonstrative.	Masc. sing. י, 346. Fem. sing. זח, 351, 353. Plur. לא, 350.
	Relative.	With antecedent י, 356, 349 (?). Without antecedent אש, 352.
<i>Nouns.</i>	Feminine ends in	ח.
	Masculine plur.	ם, 356; ן, passim.

Apparently there is one case where a noun does not take the plural ending after a numeral, 358.

The *proper names* found are מש 349, 351, 352, 353.

רם 357.

חע 351 (?).

חז 349 (?).

כר 352.

.נמ 352 (?).

ר?בם 358.

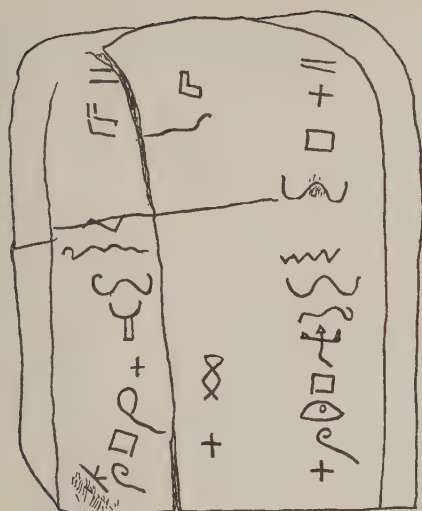
*The Three New Inscriptions*

## INSCRIPTION 356

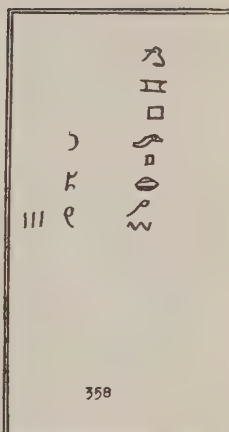
This little inscription ( $23 \times 20$  cm. at the larger end, and  $23 \times 13$  at the smaller) was found near Mine L by the Harvard Mission. The slab is not complete: on the right, the upright groove is preserved but the arch above and the bottom groove are gone. It was apparently broken along the left upright groove. The inscription consists of two short vertical columns with one letter between them on top. In places it is exceedingly difficult, even with the original, to distinguish between weather-marks and chiselled letters. Each letter is accompanied by a point, which helps in fixing the number of letters and distinguishing them from natural abrasions. The fact that a letter has been placed between the two columns, without itself forming part of a column, would tend to show that the letters on a level with it on either side are really the first of their respective columns, and that no letter is missing on top. To judge from the direction of the curve on top of the right groove, no letter is missing at the beginning of the first column and there could not be more than one missing in the left column. After the tablet broke so that the first letter of the second column was carried away, the letter was apparently written again in the free space between the two columns. The second column ends with בעלם, which occurs so often at the end of the Sinaitic inscriptions, and it is thus highly probable that nothing is missing at the bottom. Thus, as far as letters are concerned, this inscription seems to be complete.

First column. There is a doubtful letter badly disfigured by weathering and surrounded by weather-marks and breaks; enough of it is left, however, to make it probable that it was a shin. From the photograph I was inclined to take a downward line as part of the sign, while I omitted another to the right as a weather-mark; after examining the original I find that the contrary is the case. The line on the right has evidently been made by a chisel. Then comes a nun, then sign 24, which is almost surely a modification of taw. The next sign is 'ayin,





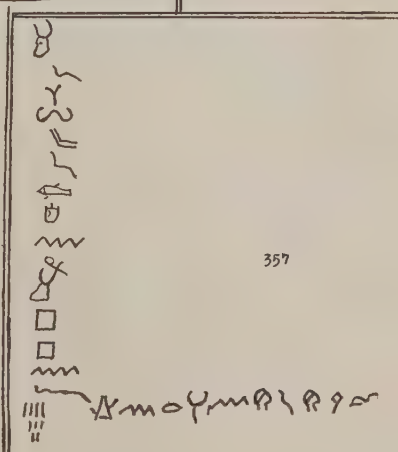
353



358



356



357

Inscriptions 353, 356, 357, 358

(for photographs see Plates IV, VI, VII, and VIII)

the dot on the right and a little below clearly excluding some lines as parts of the letter, and showing them to be mere abrasions. Then follow in order: a badly weathered nun, beth, beth, and nun; part of this last letter has been broken, but the letter is practically certain.

Between the two columns on top: mem, probable.

Second column. Two parallel vertical lines, which for a time I was inclined to take as a yod by combining it with the left stem of the mem. On closer examination it seems that the engraver intended those two lines to form a unit by themselves, and therefore placed them a little distance from the mem; I read zain, with the lines vertical instead of horizontal. As I have noted so often above (under 12, 13, 16, 18), the direction of the sign seems to have been secondary. Then comes a lamed, fairly visible on the original but hardly appearing on the photograph; then a nun followed by the usual beth, 'ayin, lamed and taw. Apparently the engraver made a mistake in writing lamed, or else the stone chipped off and he rewrote it to the left outside the column. As far as we can see, the inscription runs as follows:

Right column: ש.נתענבבנ(?)

Between columns: מ

Left column: זלנבעלת

The whole may be divided into words thus: שנת ען בבנם זלן בעלת

שנת This is probably the plural of שנה (שנת) 'sleep.' It may be also the singular of the same noun with 1st person singular suffix, 'my sleep.' The doubtful character of shin should not, however, be overlooked.

ען This I take as the 2nd person feminine singular imperative of ענה 'to answer favorably,' addressed to Ba'alat.

בבנם The first ב is evidently a preposition. בנם is probably the plural of a singular בנה = בָּנָה. This form does not occur in biblical Hebrew, but is not rare in verbs ל"ה: בָּנָה, בָּנָה, שָׁנָה, etc. Here 'buildings' or 'shelters' (built of dry stones).

זלן ז is the relative pronoun, as in the Ahiram inscription and in Phoenician; it is here construed without preposi-

tion after a verb of rest, as often. לָן (לָנָן) is a verb, 3rd person plural qal of לָן or לָן, 'to spend the night.'

בעלת Probably vocative, 'O Ba'alat.'

Translation: 'O Ba'alat, kindly answer the sleeps (or, my sleep) in the structures in which people will have spent the night'; *or*, 'O Ba'alat, in (these) shelters where people will come to spend the night, kindly answer (them during their) sleep (*or* answer [me during] my sleep).'

It is probable that here we have to deal with the erection, assumed but not explicitly mentioned, of sleeping-shelters; the one who built them placed (or rather intended to place, see below, pp. 66 f.) this tablet in or near them and asked the goddess to give an answer to her devotees when they should come to consult her by sleeping in her presence. Petrie found many such shelters near the temple and on the road to it; and it is not surprising to find a mention of them near Mine L. "We must then picture to ourselves the shrine of the XIIth dynasty as a cave in a knoll of rock, with portico before it. . . . The road to the shrine led past the head of a valley up to the cave with a line of steles along the way; while many shelters for visitors who came to dream at the holy place were scattered along the roadside farther off, with tall steles standing in them" (Flinders Petrie, 'Researches, p. 102). On the practice of sleeping near a sacred spot with a view to obtaining an answer or information on some desired subject, see Petrie, 'Researches,' pp. 66 ff., 190 f. Jacob was likewise favored with a dream at Bethel, Gen. 28, 12, and Solomon at Gibeon, 1 Kings, 3, 5 ff.

#### INSCRIPTION 357

This inscription cut on the wall of Mine L was discovered by Rev. A. W. Johnson, of the Harvard Mission; it could not be removed and the writer has had no opportunity to study the original. It was photographed by Professor Lake and copied by Professor Blake. Most of the signs, we are told, are clearly visible, but a few do not show on the photograph. In every case we have given preference to Blake's hand-copy in view

of the misleading nature of photographs of such inscriptions. The inscription consists of a vertical column and of a horizontal line meeting at right angles at the bottom of the column. The signs are carefully drawn, and for a time I was under the impression that it was more recent than the others. The difference is due only to the greater skill of the engraver; the lines themselves are the same.

Vertical column. Aleph, nun, waw, shin, gimel, nun, samekh; the eighth letter is a new sign, no. 28 of the alphabet. As has been explained above, this sign may be lamed or kaph, and its doubtful character should not be forgotten. Then comes mem with a point. The next sign (27 of the alphabet) is almost certainly lamed and aleph. Then follow in order: beth, beth, mem, and nun. At the end of the vertical column and slightly to the left are a certain number of lines, evidently a numeral. Blake counted seven; I see nine on the photograph.

Horizontal line. Right to left. This line opens with sign no. 29. I am inclined to see two letters, the second being 'ayin and the first, judging from what is left of it, being possibly pe. The other letters are all clear: lamed, resh, nun, resh, mem, waw, 'ayin, mem. The last letter is sign no. 26. As explained above, this is possibly he, but is doubtful.

Vertical column: אנושגנסמלאבבמן

Horizontal line: פ(?) עלרנרמועמה(?)

This may be divided into words as follows:

	אנוש גן סלם לאב במן
	פ(?) על רן רם ועמה(?)

אנוש If the word is vocalized אָנוֹשׁ, it is surprising to find the scriptio plena. On the other hand the possibility of a broken plural like אָנוֹשׁ must not be overlooked; morphologically this is possible, and the meaning obtained would be satisfactory. We can translate, in mining language, 'the gang.'

גן Plural after a collective. גן = גָּנוּ, 'protected,' 'guarded successfully.'

סלם If we read lamed, the meaning is 'baskets' or 'receptacles' in which the turquoise was collected and taken to the Collector or 'Uha'; if we read kaph, then we have 'the huts' or (collective) 'camp,' where the officers lived during the expedition. Either סלם or סכם gives a good sense, especially the former in view of the preposition ל which follows, and which seems to imply motion towards a place or person. However, ל may have the force of a genitive as often.

לאב ל is of course a preposition. אב, 'father,' here evidently a 'superior officer.' אב in the sense of a high official occurs in Gen. 45, 8.

במ = במנה, 'to the number.' מנה is the same formation as מנה of Inscription 356.

Then follow the nine (or seven) lines indicating so many units. Whether these refer to the number of the men in the 'gang' or to the number of 'baskets,' in case we read סלם, is not clear. If we read סכם, it would be more natural to refer the number to the men.

פעל = פעל = פעלו, 'they made.' On the use of פעל in Phoenician inscriptions see Cooke, Text-Book of North Semitic Inscriptions, p. 23. If my reading is not correct, then I do not know what this is.

רן = רן, 'jubilation,' 'celebration,' 'feast,' cf. Ps. 32, 7. There is a possibility of reading רן as a verb, רנו, and the preceding word as a noun (adverbial accusative).

רם I was at first inclined to read this word as a verb, רמו, including the following waw as part of the verb, in the 3rd person plural; but this would be the only example of a scriptio plena in the verb, and I consider it far better to take the waw as the ordinary conjunction. I am inclined to take רם as a proper name; as such it occurs in the Old Testament as one of the ancestors of David, Ruth 4, 19. See the same name in Job 32, 2, and as the son of Jerahmeel, 1 Chron. 2, 25. 27.

(?) ועמה ו is the conjunction 'and.' 'Ayin and mem are certain, but not so the last sign, as explained. If we read he,



which seems more probable, then it is likely that we have עמה = עמיו, 'his people.' At any rate it seems certain that we have here some inflectional form of עם 'people' or 'kinsman.' As stated above, we may translate עמה by 'his followers,' 'his subordinates,' or even 'compatriots.' By reading it in the plural, עמה = עמיה = עמיו, we could also translate it by 'his kinsmen,' 'his clansmen,' with a reference to the clan to which the gang belonged and of which R-M was the head.

Translation: 'The gang, consisting of nine men, successfully protected the baskets (of turquoise?) for the superior officer (sheik?); (thereupon) R-M and his people (his compatriots [?], or clansmen [?]) made a great celebration'; *or*: 'The gang, consisting of nine men, successfully protected the huts (camp) of the superior officer (sheik?); (thereupon) R-M and his people (compatriots, clansmen) made a great celebration.'

There seems to be an allusion to some raiding-party which attacked the convoy taking the baskets of turquoise to the officer, or which attacked the camp where the officer (or sheik?) lived, and where the raiders expected to find a rich booty. The raiders were repulsed by a gang of nine (seven) men. R-M, who was evidently interested in the gang, organized a celebration in which his people (or clan?) took part. The inscription may have been written by order of R-M himself. On the office held by R-M see what is said above. He may be the officer referred to in the first half of the inscription, although in that case we should expect to find his name mentioned there.

#### INSCRIPTION 358

This short inscription of eleven letters was also discovered by the Harvard Mission in Mine M, but its unfavorable position prevented its being photographed and we have only Blake's transcription. It consists of two vertical columns, one of eight letters and the other of three; to the left of the second column are three lines, evidently a numeral as in the preceding inscription. Out of the eleven letters, one is altogether unknown, and another is doubtful; the others are fairly easy to identify.

Right column. The first sign is very likely resh. The following letter is sign 30; taken by itself it could be beth or pe, but both those letters occur in the inscription with a different shape, and it is not likely that the engraver meant either of them here. Then follow in order: beth, samekh, pe, 'ayin, lamed, mem. The 'ayin has a slightly different form, but I think there is no doubt about its identity.

Second column. Gimel, in the form of a curved bracket turned to the left. The next sign is a new one, and there is some doubt as to its phonetic value. Possibly it is daleth, left unfinished perhaps by oversight or because the part actually written may have been deemed sufficient to identify it; its doubtful character should not be forgotten. The last letter is clearly a lamed. Then follow the three vertical lines to the left, evidently a numeral.

First column: ר?בס פעל מ

Second column: ל (?) מנר (ר being uncertain)

This may be divided into words thus: ||| ר?בס פעל מנרל.

ר.בס Probably a proper name, R. BS.

פעל 'Made.'

מנרל (ר being uncertain). The term מנרל designates a 'fort' or 'tower' in most of the Semitic languages. Possibly it refers here to such a structure ensuring the security of the workers against marauding Bedouins, or protecting the convoys to the sea; but this is not sure, especially on account of the uncertainty as to one of the letters. We should also expect a plural in connection with the numeral 'three,' unless we have again to deal with a broken plural.

Translation: 'R?BS made three towers.'

### *Inscriptions Previously Published*

What follows relates to the Serâbît inscriptions already known but now deposited in the Cairo Museum by the Harvard Mission and examined by me there. They are nos. 346, 349-354. No. 345 is in the British Museum; no. 347 in Brussels; no. 348 at Wady Maghareh; no. 355 escaped the eye of the

Harvard Mission and is still at Serâbît. The present writer has not had the opportunity of studying the originals of these last-named four, and consequently does not feel that he ought to attempt a translation which would add nothing to what is already known. These inscriptions 345-355 have been translated by Eisler, Bruston, and Grimme, as already stated.

#### INSCRIPTION 346

This inscription, originally in the Temple of Hathor at Serâbît is now in the Museum of Cairo, no. 38268. It is a little seated statuette of red sandstone,  $31 \times 12$  cm. The inscription is in three columns, two in front running down on each side of the lap and knees, and the third on the right side. The word between the two front columns is evidently part of the column on the left; this is shown by the fact that the letters of that column gradually bend to a horizontal direction. As noted above, Furlani examined this statuette in order to check Grimme's readings. He has given no translation, but, with very few exceptions towards the end of the inscription, the signs verified by the present writer agree perfectly with his.

Right shoulder (left of the statue). The letters seem to be perfectly clear: zain, lamed, samekh, gimel (with a point to the left), mem, beth, 'ayin, and taw.

Left shoulder (right of the statue). 'Ayin, lamed, nun. Next comes a part chipped off with about three letters missing. Then mem, taw, lamed, and, running to the right, the usual beth, 'ayin, lamed, taw.

Right side of the statue. 'Ayin, lamed, nun, 'ayin, mem followed by a point, resh, beth. Then follow six letters in three columns. The order in which they should be read might give rise to some hesitation; however, those same six letters occur in no. 349 in the following order: resh, beth, nun, şade, beth and nun. The order of the three columns is therefore boustrophedonic. Three of these six letters are accompanied by points: middle nun, şade and last beth.

Front, right shoulder: זלסגמבעת

Front, left shoulder: עלנ...מחלבעת

Right side of statue: עלנעמרבנצבן

It can be divided into words thus:  $\text{לסג מבעה על נ. . . מת לבעלה}$   
 $\text{על נעם רב נצבן}$ .

ז 'This,' demonstrative pronoun.

לסג 'For the withdrawing.' סג infinitive construct of לסג. ל preposition.

מבעה We are inclined to see in this word a pi'el participle of בעה 'to attack suddenly,' 'to fall upon.' It may refer to some Bedouin chief who was threatening the expedition with a raid, and who could very well be called מבעה.

על Preposition, here with the sense of 'according to,' 'in conformity to,' 'pursuant to.'

נ. . . מת One of the words to be supplied is probably נעם, as we find on the right side. Eisler suggests that; he also suggests that an aleph be read before מת, which would give us אמח. These are both mere guesses, but are probably right. Eisler vocalizes the second word אמח, which he renders 'oracle.' It seems to me in accordance with what we read on the right-side inscription, and in better accordance with the value of the preposition ל before בעלה, to expect some name, or title of office; I would propose אמח 'handmaid,' here probably some woman officiating in the temple of Hathor and consecrated to her service. This word occurs often in Phoenician in the composition of proper names (Lidzbarski, 'Handbuch,' p. 221).

לבעלה 'Of Ba'alat.' Eisler and Grimme take this as a dedicatory formula, 'for Ba'alat,' and not as part of the inscription, but the gradually slanting letters make it certain that it belongs to the inscription proper.

על נעם 'According to the pleasure' or 'wish.'

רב 'Master,' 'overseer,' or 'head,' as often in Phoenician inscriptions (Lidzbarski, 'Handbuch,' p. 366).

נצבן Lit. 'the setters up.' The term is probably qal participle of נצב 'to set up,' נצבן. Note the plural in נ, instead of ס as in 356. The functions of those workers was not merely to set up stelae and other monuments, but also to prepare

and inscribe them if so desired. They would correspond to our stone-engravers and stone-cutters.

Translation: 'This (statue is set up) for the withdrawal of the raider, according to the wish of the handmaid (priestess) of Ba'alat, (and) according to the wish of the head stone-setter (engraver).'

#### Previous renderings.

Eisler, p. 46:

ז לסג מרעת  
על נ[עם א]מת לבעלת  
על נעם רבן רבנם

'Dies zur Schutzwehr bzw. Umzäunung der Weide (oder Dies für die Vermehrung der Herde) nach dem Belieben eines Orakels — für die Ba'alat; nach dem Belieben des Aufsehers der Aufseher.'

Bruston, pp. 51 ff. In reading the inscription Bruston starts from the left front column downwards, then reads the intermediary Ba'alat, and finally reads the right front column upwards.

על נ[ע]מת לבעלת עתר מגף לו  
על נעם רב בננם

'À cause de faveurs à la Dame de l'abondance (= Bahalath-Hathor), de la personne de celui-ci [*i.e. de l'homme dont la figure est sculptée au-dessus*]; à cause de faveur, du chef de gens intelligents.'

Grimme, pp. 44 ff.

לבעלת  
על סגוא מרעתה  
על נ[עם בה]מת  
על נעם מנשה רבן אבנם ב?ני

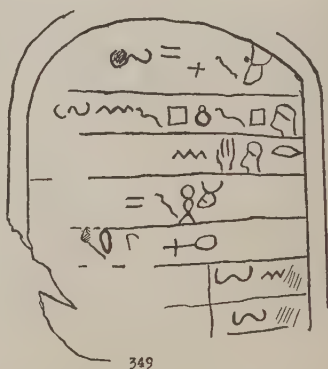
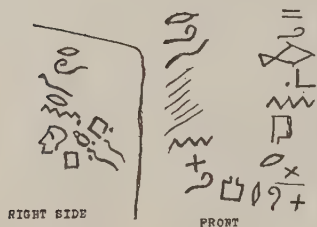
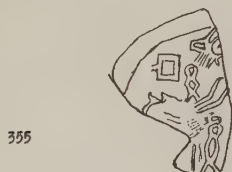
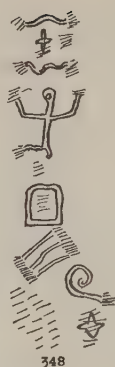
'Für Be'alet; für das Wachstum ihrer Weide; für das Wohlbe finden [der Herdentiere]; für das Wohlfinden des M(?)n(?)-š-h, Obersten der Steinarbeiter auf ?-n-j.'



## INSCRIPTION 349

As noted above, this is the inscription that has raised so much discussion since the publication of Grimme's translation. The present writer was given every opportunity at the Museum in Cairo to examine it with the utmost thoroughness. The inscription is 32 cm. high in its present state and 24 cm. wide between the upright grooves. It is in a very bad state of preservation, weathered and broken. Unlike the other inscriptions, which are written in vertical columns, this one has the lines running horizontally, and evidently from right to left. The lines are separated by a dividing continuous cut from groove to groove. In its present state there are traces of seven lines of writing, but only one is complete, while another lacks but one letter. The first words of lines six and seven are isolated by a vertical cut across the horizontal dividing lines. As to the contents, the inscription is undoubtedly obscure, but after careful examination it seems clear that most of the letters read by Grimme, and after him by Völter, are natural breaks and weather-marks, not engraved signs. It is plain to me that there is but one row of engraved letters within two dividing lines, and that all that appears above or below these letters is mere accidental markings. The great interest of the inscription lies in the fact that the name of the head stone-worker is here given, M-SH, which can be vocalized Mash (as in Gen. 10, 23), or Mosheh (since the *matres lectionis* are not written). It is needless to point out that if the reading 'Mosheh' is preferred, the official in question has nothing except the name in common with the great Lawgiver of Israel; the M-SH of the inscriptions antedates the Old Testament Moses by several hundred years.

Line 1. Aleph, nun, taw, zain, shin, with probably not more than one missing letter after the shin. The sign for shin was not recognized by Gardiner and those who came after him; see above, 'Remarks on Alphabet,' no. 19. But in the original there is no doubt about its identity.



Inscriptions 345, 346, 347, 348, 349, 355

(for photographs see Plates I and VIII)

- Line 2. Resh, beth, nun, sade, beth, nun, mem, and shin; all signs are clear, and apparently no letter missing.
- Line 3. 'Ayin, resh, yod, mem, with about four letters missing.
- Line 4. About three letters missing at the beginning, then aleph, heth, nun, zain, with possibly two letters missing at the end.
- Line 5. Perhaps four letters missing at the beginning of the line; then teth; then one, or possibly two, missing letters, followed by what looks like 'ayin and lamed, and by one missing letter.
- Lines 6 and 7. The first part of these two lines is isolated from the second half by a vertical division. Within that isolated part, line 6 has mem, shin; line 7, one missing letter and then shin. Nothing is left of what was written in the second half of these lines.

## Transcription:

- Line 1. אנתזש .
- Line 2. רבנצבנמש
- Line 3. ערימ....
- Line 4. ..אחנז...  
??
- Line 5. ט....על .
- Line 6. מש.....
- Line 7. ש.....

## Possible word-division:

אנת ז (אן תז) ש. רב נצבן משי ערימ ..... אחנז ..... ט . על . משי .....  
ש.....

After long hesitation I have come to the conclusion that this inscription, like most of the stelae, contains a prayer or petition to Ba'alat; it is, however, too fragmentary to allow any feeling of security in the rendering proposed. I submit it in the hope that others may do better.

אנת ז (אן תז) At first, I was inclined to read אן תז 'I, T-Z.' This may be correct after all; but if we follow the analogy of this series of inscriptions, it would be far preferable to read אנת, in the sense of some object to be set up. The sense would then be clearly, 'This a-n-t M-SH set up.'

Hebrew has preserved a root אנה which seems to have been connected with the making of vessels, vases, and utensils, a meaning still found in Babylonian *unutu*. From this original idea, Hebrew has אני 'fleet' and אניה 'ship.' Might we not suppose a word אנה, in the sense of some liturgical vessel or basin with a base so that it could be 'set up'? Petrie found several objects which might answer the purpose, varying from libation-altars and vase-altars to ordinary trays. The libation-altar or vase-altar would be particularly fitting on account of the following verb. "A larger and more elaborate altar was found in the Sacred Cave. It has been much broken about the top, but it had originally a basin hollow about 9 inches wide and 4 inches deep, which might perhaps have been for libation" ('Researches,' pp. 133 f.; on the vase-altars common during the XIIth dynasty, *ibid.*, p. 134). In this connection one might also think of some ablution-basin, several of which have been found at Serâbît ('Researches,' pp. 85 f., 105) and of the 'sea' connected with the temple of Jerusalem, 1 Kings 7, 23 ff., etc.

ש. Here apparently we need a verb of two radicals and beginning with shin. It is very probable that the same verb that occurs in 352, שח, is the one to be supplied here. If we read חו, then the verb would be in the 1st person singular. No other verb beginning with shin could in two radicals give us that form except שח with the contraction of the two taws. Since the object would then be a person, we should translate it by 'appointed.'

רב נצבן 'Head of the stele-setters,' as in 346.

מש Proper Name, M-SH; this official is mentioned several times in this series. מש is evidently in apposition to רב נצבן. According to the more general practice מש should precede, for רב נצבן is really the second accusative; but examples of the order of words here found are plentiful.

ער Here the prayer to Ba'alat seems to begin, and we take this word as an imperative of ער 'to arise,' addressed to Ba'alat.

ים Probably 'day'; here 'now,' 'immediately.'

..... This was probably followed by the specific prayer to Ba'alat.

אחנז. Toward the middle of the prayer occur these four letters. The combination אַחנז, 'I shall favor the one who,' does not seem to fit; on the other hand there is no trilateral word the last two radicals of which would be aleph, heth; hence there is little doubt that we should read אח 'brother.' The following word is not complete, but there are only four or five roots that begin with nun, zayin, and only one that gives any sense here. So here again I feel tolerably safe in reading נזר 'one consecrated, devotee,' and hence 'prince.' 'The brother of the prince' or 'the brother of thy devotee' אַח נזר.

ט. על. Whatever be the word to which the last two letters belong, it is well nigh certain that they form the end of the prayer proper. Lines 6 and 7 have received a special treatment as stated above, and have been isolated from the rest; they seem to contain some kind of signature. If so, I am strongly tempted to read here the usual בעלת which occurs at the end of so many of our inscriptions. It would be here, as elsewhere, a vocative: 'O Ba'alat!' The preceding teth is evidently the last letter of a word that cannot be identified but might be מלטי = מלט 'save'; שפט 'judge'; or some such word.

מש... In line 6 we have again the name M-SH, and very ש... likely also in line 7. What was written in the second half of the lines is entirely obliterated.

Translation: 'M-SH, head of the stele-setters, has erected this libation(vase)-altar (?); arise, now, O Ba'alat . . . the brother of the prince (of thy devotee) . . . M-SH . . . -SH . . . .'; or 'I, T-Z, have appointed M-SH as head of the stele-setters; arise now . . . etc.

#### Previous renderings.

Eisler, p. 38.

1	אן טזן	4	אחז	'Ich, Tzes, Vorsteher der Steine
2	רבן אבנם	5	צט	(so and so viel) Mässchen (Se'im)
3	שאים	6	ש	7 ש . . . ausgesucht . . .'



Bruston, pp. 55 f.

1	אן טנם	'Je suis Tannim (Dragon) fils du
2	בן אבן מיש	fils (petit-fils) de Mesha' 'a'am
3	עאעם	(libérateur du peuple).'

Grimme, pp. 63 ff. (Völter's rendering is but little modified from that of Grimme.)

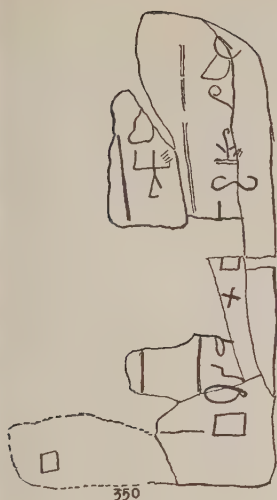
1	אנ חיתשפשוחנמימנמ.	'Ich bin Hjtšpšwḥnm·m·m·
2	רבנ אבנמ...	Oberster der Minenarbeiter (vom Sinai?)
3	סר בת מאנה ? יהו סנ?	Hauptmann des Tempels der Ma'na(h) [und] des Jahu [vom] Si[n]ai
4	מא. בבקלת חיתשפשוחנמימ.	(Du,) Me'[oh]a[b]-Be['a]let Hjtš(?)pšw(?)ḥn(?)mjm
5	נעמ (משחונ) משחונ מנ יא?ר	Du warst freundlich, hast mich gezogen (gegriffen) aus dem Nile
6	ו.ס... מסב מ.....	Und [du hast mich gesetzt über] den Aussentempel d[er] M.
7	אשר..... בסני	Welcher auf Sinai [ist].'

#### INSCRIPTION 350

This inscription is in several small fragments and far from complete. Originally it probably had three columns, but only the first column can be reconstructed, and that not entirely. It is impossible to give the dimensions, but it must have been about 30 cm. high.

First column.

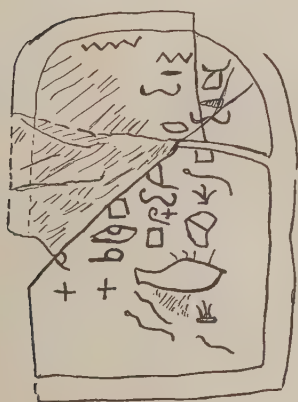
Top fragment. Aleph, lamed are certain; then comes a letter which is not clear but seems to be our sign no. 25, with the phonetic value of kaph (Eisler reads it as he). Then a shin, which is certain. The next letter is not complete, the visible portion consists of a vertical line which could belong only to taw or to gimel. It is more probably gimel, since the vertical line of taw would be nearer the centre of the column and not so much to the left as it is.



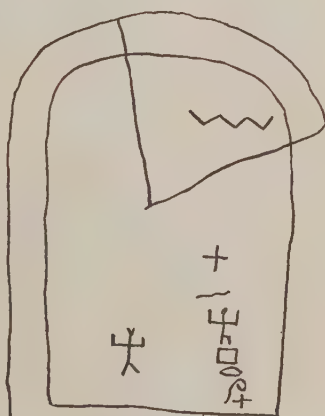
350



351



352



354

Inscriptions 350, 351, 352, 354

(for photographs see Plates II, III, and V)

Middle fragment. Beth and taw. The beth follows immediately the incomplete letter of the top fragment.

Bottom fragment. Part of a letter that cannot be identified; it consists of a loop the top of which is broken off. It must of course belong to a sign with an oval loop, and this appears only in a few letters, notably 'ayin and šade. This letter is more probably 'ayin, as the loop is a little too narrow for šade. Then follow in order nun, šade, and beth.

Second column.

There must have been a few letters in a top fragment now lost; in the extant fragment are inscribed aleph, he or heth.

Third column.

In a fragment formerly attached to the bottom fragment of the first column, but now detached, stands a beth.

The inscription consists of the following letters:

First column א ל ב ש ? ב ת ? נ צ ב

Second column א (ח) ה

Third column ב

Possible word-division: א ל ב ש ? ב ת ? נ צ ב . . . . . א ה . . . . . ב

אל 'These,' demonstrative pronoun. In this and other cases, it is impossible to know what the demonstrative pronoun refers to; it may have been a certain number of stelae, or perhaps sleeping-enclosures. The verb used will indicate in a general way the nature of the structure. Here the verb is apparently נצב 'to set up,' and probably refers to upright stones, as there are many at Serâbit.

כש?ב As explained above, the partially preserved letter is probably gimel, giving us כשנב. כ seems to be the preposition before an infinitive construct with the force of a temporal sentence, referring, as usual, to time prior to that of the main verb. שנב might be שִׁנֵּב 'to be exalted,' 'to be highly successful,' the following noun being the logical subject.

ח? The incomplete letter is probably 'ayin (possibly sade), giving us חע (הצ). חע is a proper name, 2 Sam. 8, 9. 10; 1 Chron. 18, 9. 10.

נצב Probably a verb, qal, 3rd person singular, 'he set up.'

No meaning can be suggested for the letters of the second and third columns.

Translation: 'T-', having been highly successful, set these up. . . .'; or, 'After his elevation (appointment to a high office) T- set these up.'

### Previous renderings.

Eisler, pp. 99 ff.

אלה שה.. בתל נצב  
נשה.....פ

'Diese (scil. Zeichen o. dgl.) hat N. N. der Ausgesonderte (Gewählte oder aktiv "der Ausscheider," Metallscheidemeister, oder Türkis Sortierer) aufgestellt (= geweiht) eingedenk (seines Gelübdes o. dgl.).'

Bruston, pp. 63 ff. (Bruston laid the fragments on their side and read from right to left; but the fragments were not put in their true relative position).

בשנע התבל עלו את נשן  
(ideogram) העז

(Prière pour Goshen)

'Quand le monde est furieux contre lui, mets en sûreté Goshen [ideogram: homme et plante].'

Grimme, p. 61.

אלו מנשה.. בת.. ה נצב  
יה אהל א

'Diese (plur.) hat M-n(?)-š-h [der Hauptmann] des Tempels von [...] h errichtet. [...] die Familie (?).'

### INSCRIPTION 351

This inscription, 32 × 22 cm., consists of two fragments which fit together almost perfectly; it comprises two vertical columns on the left half of the slab, the right being occupied by

the representation of Ptaḥ. Originally it must have contained about 24 letters, but some in the left column have been worn out. Many of the letters seem to have been accompanied by a point, but this does not occur regularly as in 356; moreover, some of the points may be due to the action of the weather. When found it was on the same slab as 353, see below.

First column. Zain, taw; it is not certain that the two lines of the zain are joined by a cross-stroke, and the transversal bar of the taw is a little longer than usual, but both letters are certain. Then beth. Below the beth a section has been chipped off at this point probably before the slab was inscribed; this chipping off widens, and extends to the edge on the left, but no letter seems to have been written in that space in the right column. On the photograph this scaling has the appearance of a fish. To the right of it a sign resembling taw also appears on the photograph, but when examined on the original it seems to be a weather-mark. Then follow in order: nun, mem, shin, nun, ṣade, beth, qoph, and ṭeth. All these signs are clear, and the only doubt is as to the phonetic value of some of them.

Second column. The first letter is badly damaged, but from the outline it must be either resh or lamed, more probably resh. The next letter is clearly mem. There is room for one or two letters in the abraded section. Below the abrasion are two doubtful letters badly worn; then probably a nun followed by another worn letter. Then follows what is very probably the bottom of beth, then 'ayin partly destroyed by a break, finally lamed and taw, both certain.

First column זחבנמשנצבקט

Second column רמ....נ.בעלת

Possible division of words:

זח בן מש נצב קטר מ....נ. בעלת

- זח Demonstrative pronoun, feminine singular, 'this'; here again it is hard to know to what it refers, but evidently it was something 'built,' as the following verb indicates.
- בן This we take as a verb, 3rd person masculine singular from בנה defectively written, 'built.' It could be taken



as part of the subject: בן מֶשׁ, 'Son of M-SH.' with the following נָצַב understood as a verb in the perfect; but the order of words points to בן as a verb followed by its subject, in accordance with Semitic usage.

מֶשׁ Proper name, already explained.

נָצַב This we take as a qal participle of נָצַב 'setting up.' Secondary statements after a finite verb are often in the form of a participial clause.

קָטַר The resh of this word is not certain, as already stated; but it seems to be the only letter that fits both the outline of the letter on the slab and the context of the inscription. It is evident that M-SH set up or erected something; consequently a lamed in place of resh would not yield any sense. I am inclined to find here a reference to an altar of incense, of which there are several instances at Serâbît (Flinders Petrie, 'Researches,' p. 189). How we should point the word is very doubtful; possibly it is of the qitâl formation, which in primitive Semitic was often used for the instrument, or vessel (Barth, 'Nominalbildung,' § 42), and in Hebrew would be קָטֹר (the feminine קָטֹרָה occurs Deut. 33, 10).

מ. Of the word which must have stood here, only the first letter remains.

... It is impossible to reconstruct the missing letters with any satisfaction.

בַּעֲלָה 'Ba'alat,' as usual.

Translation: 'This M-SH built, setting up an altar . . . Ba'alat.'

#### Previous renderings.

Eisler, p. 101.

זֶה בְּנִשְׁמֶשׁ נָצַב קָטַר[. . ח] 'Dies hat Ben Shemesh aufgestellt, weihräu[chernd dem . . . als Sü]hnopfer.

Bruston, pp. 57 f. (Bruston, after laying the inscription on its side, read from right to left, thus inverting the order of letters).

בְּצֵן שֶׁמֶשׁ נִבְחַת חֶקֶן 'Quand le soleil pique, rétablis les productions de celui-ci [*i.e.* du personnage figuré au début].'

Grimme, pp. 71 ff. (Grimme held this inscription to be a palimpsest, the old inscription [A] having been erased and a new one [B] substituted.)

## B

זת ביסתהמש נחץ בחק טחותמש

‘Dieses hat Bjust (?) hmš (= Bjaste-mose) abgekratzt auf Verordnung des Thwtm(?)š (= Thutmose).’

## A

— פשוח .. ימנמ?

— פשוחנמ.מ.מ?

## INSCRIPTION 352

This inscription, 30 × 19 cm., consisted of two main fragments at the time of Gardiner; since his day a portion of the top fragment has been broken off, but all the letters are still clear. It consisted originally of four vertical columns, at least in the lower half. The lower half is well preserved, but the upper section is badly worn and weathered.

First column. Aleph, followed by a short horizontal incision which seems to be artificial, not mere natural weathering, but, since it has no special shape, almost certainly does not stand for a letter. It is probable that as the engraver was writing the second sign the stone chipped and he rewrote below. Then follows shin. In the lower half, beth; the letter is certain although only the bottom part is now visible. No other letter stood between this beth and the shin of the upper half. There follow nun, kaph, resh (as explained above), samekh, kaph, and nun.

Second column. Mem, under which appears an accidental incision, not a letter; then shin and ‘ayin. There must be one missing letter between ‘ayin and the first letter of the lower fragment; it is probably a lamed with the top of the loop broken off. Then shin, taw, and lamed. It is evident that the taw was forgotten, and inserted later by the engraver, naturally in a smaller size. Then beth, nun, nun.

Third column. Mem. In the upper fragment all the rest is obliterated. In the lower fragment the usual בעלת.

Fourth column, lower fragment. Lamed and tau, probably again בעלת.

First column אש בנ כר סכנ

Second column מש ע. לשח לבננ

Third column מ. בעלת . . . . .

Fourth column לת . . . . .

Possible division of words:

אש בן כר סכן מש ע. ל שח (לבננם, לבנן מ.) לבן נמ. . . . . בעלת. . . . . לת

אש Relative pronoun, 'that which.'

בן Apparently 3rd person perfect of בנה, 'built.'

כר Proper name, subject of בן, K-R.

סכן Probably participle of סכן, 'to take care of'; here in the sense of 'steward'; cp. Is. 22, 15, also the Aḥiram inscription and the glosses in the Tell-el-Amarna tablets (KAT<sup>3</sup>, p. 652).

מש Proper name, M-SH, as above.

ע. ל This word seems to be the object of the following שח 'set up.' It evidently means some object that can be set up, unless we understand it as a proper name and translate שח by 'appoint.' This latter rendering is not likely in the context. Of all the roots beginning with 'ayin and ending with lamed, we find only one that gives sense here. viz. עגל: 'He set up an 'egel.' Should this word be understood as a calf-idol or calf-symbol? It is not certain, for Petrie did not find any such objects at Serâbît; still, the site was not systematically explored and it is possible that such figures may yet turn up. Perhaps עגל should be taken in its etymological sense of 'to be round,' and we should render 'a circle.' It might refer to the semi-circular rings of stones around stelae ('Researches,' p. 65), or possibly tomb-circles (ibid., p. 249). If so, K-R built some kind of a structure around which a circle of stones was set up. At any rate the object in question seems to be connected with the structure of K-R; otherwise it would not have been mentioned in the same inscription; see also below.

שֶׁת Probably 3rd person perfect of שָׁת 'to set up, erect.'  
 ... נִבְנָה (לִבְנָה, לִבְנָה מִ... לִבְנָה נִבְנָה). It is here exceedingly difficult to know the exact syntactical relation of the words to one another, owing to the missing words that follow; consequently it is also very difficult to know the meaning intended. לִ is of course a preposition. On the whole it seems more natural to take נִבְנָה as a proper name, subject of שָׁת, and בִּן in the sense of 'building' or 'structure'; apparently the form בִּנָּה (בִּנָּה) occurs in 356. The meaning would then be: 'N-M-? set up an 'egel for the building.' This is not the only possible construction or translation; בִּן may be taken in the sense of 'son' or (construct plural) 'sons of,' 'family of,' and N-M- taken as a genitive, so that we could translate: 'He (K-R) set up an 'egel for the son (sons, family) of N-M-.' Again, we may read בִּנָּה in the absolute plural '(sons),' 'builders' or 'buildings'; the following mem might then be the first letter of a following word, perhaps a proper name. Or finally, בִּנָּה might be read as a plural noun with suffix 3rd person plural, as in Phoenician (see Lidzbarski, 'Handbuch,' p. 396); 'their sons (builders, buildings),' that is, the sons of K-R and of his master M-SH. But this does not seem to fit so well in the context.

בַּעֲלָה 'Ba'alat.'

לִה... Probably part of the word 'Ba'alat.'

Translation: '(Structure) which K-R, steward of M-SH, built; N-M-? set up a calf-idol (ring of stones) for the structure . . . Ba'alat . . . (Ba'a)lat'; *or*, '(Structure) which K-R, steward of M-SH, built; he set up a calf (ring of stones) for (the benefit of) the son of N-M-?.'

On other possible translations, see above.

#### Previous renderings.

Eisler, p. 96.

אֵן . . . נָנוּ לִסְכָּן

פִּשְׁתָּלֶפֶת

בַּעֲלָה

'Ich..heimste ein zur Läuterung  
 oder Siebung (so and so viel).....'

Bruston, pp. 58 ff. (Bruston considered the two halves sepa-

rately. He laid the slab on its side, and read from right to left, again beginning at the end of the columns. The sign for fish he considers as an ideogram for 'tree.' In his view the inscription is a prayer for a tree.)

Lower half.

רתן ז

בטש ו

ננתך עב

ח

'Étends ce genévrier et qu'il se répande (ou s'étale) en forêts.'

Upper half.

לנאם שמש

'Pour un oracle du (dieu) Soleil.'

Grimme, pp. 57 ff. (Grimme also considers the two halves as distinct inscriptions.)

Upper half.

אנ סנהשמה בנ .. חש ...

'Ich bin S-n-h-š-m-h, Sohn des .j(?)-t(?)-š...'

Lower half.

גני על גשלת בסני בערת הנח (הנחם) 'Mein Grab ist auf der Spitze in Sinai, innerhalb der Gemeinde der Ruhe(-nden?).'

#### INSCRIPTION 353

This inscription, 40 × 27 cm. inside the grooves, consists of three vertical columns; it seems to have been purposely defaced by means of a chisel, the second column particularly having suffered. A photograph in such a case is entirely untrustworthy, but even the original offers many difficulties. As often, the columns are separated by a vertical dividing line, but they must have been added after the letters were written, for some of the letters are cut by the dividing lines. It will be remembered that originally this inscription was on the same slab as Inscription 351 but running in the opposite direction. From the photograph of Gardiner in his article (p. 16, plate V) I should infer that 354 also was on the same block when found by Petrie. Evidently the engraver's habit was to prepare several of these inscriptions, and then to separate them afterwards. Why



these were not separated at the time, we cannot say. Could it be that these were rejected? If so, the better specimens may still be in place in some unsearched enclosures.

First column. Zain, taw, and beth are clear; the next sign, which on the photograph looks like pe, is probably shin; the following letter is almost entirely obliterated. Then follow mem, shin, a variety of nun (possibly mem), he or heth, then בעלה.

Second column. Gardiner and after him Eisler and Grimme give a sign on top of the column which seems to them to look like a scarab; some markings are to be found there, but it does not seem that we have to do with an engraved letter. Only two letters are visible at the top of the second column, gimel and nun. I could not identify anything else in that column except heth and taw at the end of it.

Third column. Zain, gimel, two letters that cannot be identified, then a variety of nun (mem?), shin, waw, taw, lamed, beth, lamed, and kaph. What Gardiner gives as an engraved sign to the left of kaph is mere weather-marking, except the line under the kaph. This seems to me the base or ground of that letter; see 'Remarks on the Alphabet,' no. 11.

First column זחבש.משנ(ה)חבעלת

Second column גנ.....חת

Third column זג..(מ)נשותלב לב

Possible division of words:

זחבש.משנחבעלתגנ.....חתזג..ןשותלב לב

The translation is exceedingly difficult, and I hesitate to present an attempt.

זח Evidently the demonstrative pronoun, 'this.'

בש Here we should expect a verb, but it is almost impossible to find one with beth and shin as first radicals that would give any satisfaction. True, the shin is not absolutely certain; but it is probable, and to put another letter in its place would be merely to abandon definite probability for a mere speculation. Accordingly we must retain the shin. We could hardly suppose a verb like Assyrian *baš-*

*amu*, 'to make,' 'to set up'; it is therefore more natural to take ב as a preposition followed by a biliteral word: בשם 'in the name of,' or some such word.

- מש M-SH, probably a proper name.
- נח Possibly a verb, 3rd person of נח 'to rest,' perhaps here in the sense of 'to be placed,' 'deposited.' The hiph'il of this verb occurs with the sense of 'set up,' 'place,' Joshua 4, 3; 2 Kings 17, 29, and also probably in Phoenician, CIS, I, no. 118; so here, 'has been placed.' The position of the verb at the end of the clause is not impossible. If we accept a verb בשם 'to set up,' the possibility might be suggested of reading here an imperative, addressed to Ba'alat, נח from נחה 'lead,' 'direct the eyes,' the object of the verb, 'eyes,' being understood, as after ברת, שתט, etc.; it would then be a deprecatory expression: 'Turn (thine eyes).'

בעלת 'Ba'alat.' Here probably vocative, since the last part of the inscription seems to be an address to the goddess.

נן Possibly imperative, address to Ba'alat; 'protect' = נָנִי.

חת . . . . . ?

ז Relative pronoun, 'which' or 'who,' referring to the missing words of column 2.

נ . . . 'Have . . . me.' There is a probability that before the nun there stood shin; part of that letter is still (doubtfully) visible. If so, a fair conjecture would be שְׁנִי = נִשְׁנִי 'have driven me away,' 'dispossessed me.'

שות The *matres lectionis* not being written, we hardly have here a form of the verb שית 'to set up,' but rather a verb middle-waw. I think it possible to read שְׁוֶת, pi'el, 'I have set,' from שוה.

לב Probably לבי 'my heart.'

לך 'To thee,' referring to the goddess: 'I have set my heart to thee,' that is, 'My hope is in thee.'

It would seem that M-SH and the members of his band at Serâbît had suffered some misfortune for which the help of the goddess was sought. Either the names or the nature of the trouble may have been given in the second column, and

that may be the reason why this column was deliberately chiselled out.

Translation: 'This has been placed in the . . . of M-SH; O Ba'alat! protect . . . who have . . . me, I have set my heart to thee (my hope is in thee)'; or, 'M-SH set this up; turn [thine eyes], O Ba'alat! protect . . .' etc.

### Previous renderings.

Eisler, pp. 60 ff.

ז(ה)ת בפם שנה בעלת  
גנזים  
זג. . . . בתל בלכין

'Dies auf Befehl der Sinah, Her-  
rin der Schätze . . . Aufseher  
der Qeniter.'

Bruston, pp. 61 ff. Ex-voto pour les jeunes gens de Goshen.

זת בקבם שנת בעלת  
אננו ולפן צלתה תחת  
כל עלל לגשן נתתה

'Ceci, dans les salles qu'habite la  
Dame de ce qui est caché et de-  
vant son ombre (protection), à la  
place de tout jeune homme de  
Goshen je l'ai donné.'

Grimme, p. 52 (from the fact [?] that the middle column is higher Grimme reads it first, then the left column, and finally the right column).

ו גנז יוסף . . . פד ..  
זה חק מ.שה ומונה למכם  
זה בספת מסך מאנה בעלת תם

'Wehe! beigesetzt ist Joseph,  
(Soh- event. vo)n·p-d. .  
Dieser hier ist der Steinschrei-  
ber (-metz?) des M-?-š-h und der  
Zähler der Tempelabgaben.

Dieser hier ist auf der Schwelle  
des Pronaos der Ma'na Be'alet  
umgekommen.'

### INSCRIPTION 354

This inscription is now in four fragments; originally it must have consisted of two columns. The left column contains but one letter at the bottom, either ה or ח. The column on the right begins with mem followed perhaps by nun. At the bottom is the usual בעלת preceded by taw and he, or ḥeth.

Nothing else is left, and even the fragment containing the last-named letters, taw and he, has been separated and is not at the Museum of Cairo.

מנ.....תה בעלת  
ח(ה).....

No translation is possible.

### Previous renderings.

Eisler, p. 49, reads 'Ba'alat,' and suggests the possibility of reconstituting the missing word by comparing with Inscription 353; he does not offer any special translation.

Bruston, p. 66.

נ(ת)נת לה פז ז

'Elle lui a donné cet or fin.'

Grimme, pp. 55 f.

מאנה בעלת  
ת.....

. . . (der) Ma'na Be'alet (um-  
gekommen?).'

### INSCRIPTIONS 345, 347, 348, AND 355

Inscriptions 345, 347, 348, and 355 have not been examined by the present writer; and without access to the originals he feels unable to add to what is already known. The following comments can, however, be made.

The first line of Inscription 345 is read and translated both by Eisler, p. 31, and by Grimme, p. 42, מאהב = מאהבעלת 'beloved of Ba'alat'; the reading and translation seem to be correct as against Bruston, p. 65, who reads ידו הבעלת = a proper name.

The second line offers more difficulties. Eisler, reads it יד 'monument to Ba'alat'; Grimme, יחור לבעלת 'special (gift) to Ba'alat'; Bruston, יהבו לבעלת 'has given it to Ba'alat.' Bruston is certainly wrong in reading יהבו, for these letters do not seem to be there. Against Eisler's reading there are two objections: (1) it is not likely that the holem would be fully written in a word like יד, and (2) the distance between the waw and the following daleth would lead us to suppose that something is missing there. Grimme has inserted another

letter and reads יחור. But the form given here to ḥeth does not occur elsewhere in our inscriptions; what he reads as waw is more than doubtful, and does not occur in our inscriptions. Moreover, I have doubts as to the first sign being yod at all, and am inclined, with Sethe, to take it as kaph. At any rate, between the first letter and the daleth there is room for two narrow letters, or for one letter wider than waw. If the first sign is really kaph, then we should have a word beginning with kaph and ending in daleth; could it be ככר? In that case, part of what appears now to be a waw would be due to natural weathering. But nothing certain can be said until the original is thoroughly examined.

In Inscription 347 the letters are correctly read by all as נתנה.

In Inscription 348 Eisler, p. 45, reads נתנה בעלת 'Ba'alat gave' = 'generous Ba'alat.' Bruston, p. 51, finds the same reading, but considers the phrase as a proper name, the counterpart of נתנאל. Grimme, p. 76, also reads and translates like Bruston, but suggests the possibility of reading מאנה בעלת. For the present, I have no substitute rendering to offer.

Inscription 355 is still at Serâbît. I have no criticism or suggestion to make; see the renderings of Bruston, pp. 66 f., and of Grimme, p. 56. This inscription will surely be recovered some day, and possibly with its missing fragments.

We thus reach the following conclusion with regard to the inscriptions of Serâbît regardless of their date and origin.

The sphinx (345) and the statuettes (346, 347) were found in or near the temple of Ba'alat at Serâbît; they seem to be votive offerings. The inscriptions carved on the walls of mines L and M (357, 358) commemorate some secular events and have no religious import, at least from our point of view. The stelae begin by commemorating, implicitly (356) or explicitly (350, 351, 352, 353, probably also 349), the erection of some structure or monument and end with an invocation to Ba'alat.<sup>48</sup> It is a remarkable coincidence that all these commemorative stelae should have been found in front of Mine L. We remarked above that nos. 351, 353, and 354 were found on

<sup>48</sup> As to Nos. 354 and 355, nothing can be said, because of our lack of acquaintance with their contents.



one large slab. They were evidently engraved near the spot where they were found, for if they had been brought from elsewhere they would first have been separated to make transportation easier. Further, as just noticed, the stelae are commemorative tablets of the erection of some structure, which is often designated only by a demonstrative or by a relative pronoun, 'this,' 'these,' 'that which.' Those who had the inscriptions made did not feel the need of describing the structure in detail because the tablet was to be placed in or near it. Therefore none of these tablets is really in the place for which it was destined and inscribed. The natural conclusion is that near Mine L was some sort of a workshop in which the stelae were being prepared.<sup>49</sup>

A further problem arises. Why have not these stelae been carried to the place where they belonged? Could it be that they are rejects, discarded and sometimes defaced because they were not considered good enough? Our hopes run high at the thought that the good tablets which passed inspection may still be in their proper places, scattered over the ridges of Serâ-bit, buried under fallen enclosures or shelters.

[NOTE. Of the photographs reproduced in the plates the following were taken by the Egyptian Museum at Cairo: Plate II (a) and (b) showing Inscriptions 349, 350; and Plate VIII (b) and (c) showing Inscription 345; the other photographs here reproduced were taken on Mt. Serâbit in 1927 by the members of the expedition. For the drawings of the inscriptions on pp. 37, 48, and 53, and the Table of Alphabets (opposite p. 1), Professor Butin is responsible.]

<sup>49</sup> It is hard to form an opinion on the relation of this workshop to Mine L. Was the mine under operation when the tablets were written? Had it been closed, or had it not yet been opened? If we knew the exact position of this workshop we might venture an opinion, but the front of the mine has evidently crumbled down (Petrie, *Researches*, p. 130). Our tablets lie among fallen rocks, and they themselves may also have fallen from the cliff above the mine; if so, they may have been there long before the mine was in operation.



# THE VULGATE, PESHITTO, SAHIDIC, AND BOHAIRIC VERSIONS OF ACTS AND THE GREEK MANUSCRIPTS <sup>1</sup>

JAMES HARDY ROPES

HARVARD UNIVERSITY

AND

WILLIAM H. P. HATCH

EPISCOPAL THEOLOGICAL SCHOOL, CAMBRIDGE

THE use of statistics in textual criticism is always attractive to the student, but is apt to be disappointing in application. It resembles the attempt of Raymond Lull in the thirteenth century to convert the Mohammedans by a mathematical demonstration of the truths of Christianity: in both cases the insights have been chiefly gained through other and more direct processes, and the figures can seldom do more than provide interesting illustration, or the test of an hypothesis; they seldom lead to much new knowledge. Moreover, it is very difficult to make the statistics either complete or perfectly accurate. Nevertheless, in textual criticism something can be learned from statistics both by way of verification and of suggestion; and the following study of the chief versions of the Book of Acts seems to the writers to yield some profitable fruit.<sup>2</sup>

In presenting the figures, especially those relating to the Vulgate, we are uncomfortably aware that, although pains have been taken with them, they are approximate only, and not perfectly accurate. But to have made them perfect would have led to no useful result, for the margin of error is very small and is incapable of affecting any general conclusions that it is proper to

<sup>1</sup> For the investigation of the Vulgate in this article Professor Ropes is responsible; the collation of the Peshitto, Sahidic, and Bohairic versions has been made by Professor Hatch.

<sup>2</sup> Yet one motive in publishing this paper is to call the attention especially of younger scholars to the extreme caution which is necessary in trying to draw deductions from text-critical statistics.

draw. In such a study it is only the broad relations of sum-totals and the unmistakable lessons of the statistics that have significance. Slight numerical differences in these complicated matters, where so many possible causes may have been at work, lead to no just inference and never deserve any consideration whatever. That is especially the case in dealing with series of figures produced by considering only those Greek readings which the accident of a foreign idiom is capable of rendering. A large (though not a predominant) element of chance is introduced by the principle of selection necessarily adopted.

In the volume entitled "The Text of Acts" (The Beginnings of Christianity, Vol. III) by J. H. Ropes, four Appendices present in tables (pp. 276-371) an analysis of the textual structure of the four great versions of Acts — Vulgate, Peshitto, Sahidic, Bohairic. In these tables all the readings of each version which seem to show an underlying Greek text different from that of Codex Vaticanus are set forth, and are classified in analytical columns in accordance with their relationship to the three chief types of text — that of the Old Uncials, the Antiochian, and the 'Western' text (chiefly as represented imperfectly by Codex Bezae). The facts there given are instructive, and may well furnish material for further study looking to a more trustworthy estimate of the value of the several versions in the criticism of the text. But such a method of presentation has the obvious defect (emphasized in the prefaces to the tables) that it does not show how far the several versions are in fact dependent on the text of Codex Vaticanus, but only to what extent they may possibly be so dependent. A rendering not mentioned in the tables may be a case where clearly the version has followed the text of B, or it may be a case where the translator would have given the same rendering, no matter which of two rival Greek readings known to us had lain before him. In order to show the full relation of a version to the several Greek authorities a further careful study is needed by which the cases would be noted and assembled in which the translator was in all probability actually following the text of B. The tables of the book show the cases where he was definitely not doing so.

In all other instances than those of these two classes the renderings are indeterminate, and useless for the inquiry.

For the 'Western' text such an examination would not seem to be very important, unless for some special reason it were desired to show precisely the proportion of underlying 'Western' Greek readings in any version. The differences of the 'Western' from the non-western Greek are of such a striking character that a very large proportion of the renderings which do not depart from B can be assumed to depart definitely from the 'Western' reading. Thus, for instance, a glance at the tables will show that the Bohairic version has probably a 'Western' element, but at best an extremely small one; and a determination of the exact percentage would make no appreciable contribution to our knowledge. On the other hand, a cursory inspection of the tables for the Peshitto shows at once that that version contains many unquestionable and important 'Western' readings; but here again the exact proportion of these to those of a different origin, reduced to percentages, would not in itself form a valuable piece of information, for what is needed is the detailed study of the character and probable origin of this admittedly important 'Western' element. It may have been already present in a mixed Greek text used by Rabbula's translators. If so, was that mixture due to deliberate selection or to carelessly overlooked survivals? On the other hand, the Syriac revisers, working on an Old Syriac version of 'Western' character, may have adopted by their own lights from a variety of sources the Greek readings to be rendered, somewhat after the fashion of the Company of Revisers of the English New Testament of 1881. Or are other possibilities conceivable? And has all this any bearing on the question of the Greek text or texts used in Antioch before A.D. 400? Any scholar working at such questions as these would need to know, not only which are the Peshitto renderings in which the Greek text implied was different from that of B and its allies, but also how extensively the Peshitto renderings positively imply the use of the text of B. But in order that this information should be valuable to him, he would have to get it for himself, by the use of his own observation and judgment in detail. Mere



statistics made by someone else would not help him much. In the cases also of the Vulgate and the Sahidic, in both of which versions the existence of a 'Western' element is plain, statistics would not be likely to throw light on the real problems.

Of the question of the relation of these versions to the Antiochian text, something will be said below, although we cannot pretend to have treated it adequately.

The field, however, in which this supplementary and statistical inquiry is mainly interesting is that of the five chief witnesses to the Old Uncial text — Codices B ~~N~~ A C 81. These manuscripts unquestionably represent a single type of text, but are not very closely related to one another within that type, and an examination of the relation of the versions to them severally may throw light on the general problem both of the codices and of the versions. In this inquiry attention is properly limited to those sections of Acts, comprising nearly one half of the book, and well distributed within it, in which all five of the Old Uncial authorities are extant. The sections are as follows: Acts i. 2-iv. 3; vii. 17-x. 42; xiii. 1-xvi. 36; xxiii. 9-18; xxiv. 15-xxvi. 19; xxvii. 16-xxviii. 4. The whole body of text contained in these sections has been studied for the Vulgate; for the Peshitto, Sahidic, and Bohairic the investigation was limited to Acts i. 2-iv. 3; vii. 17-x. 42 (comprising rather more than one quarter of the whole book).

## I

### VULGATE <sup>3</sup>

In spite of the obvious fact that the Vulgate Latin version is in its main structure non-western, a considerable number of 'Western' readings are found all through it, as is apparent from a glance at the tables in "The Text of Acts." A part of these are wholly unimportant and were probably coincidences, due to the working of a motive in the mind of the reviser who produced the 'Western' text similar to that which influenced the translator from the Greek. In many respects an amplifying reviser and a translator tend independently to make the same changes

<sup>3</sup> Wordsworth and White's text, 1905, has been used for collation.

and, especially, additions. Further, some of these *prima facie* 'Western' readings in the Vulgate, particularly as respects the order of words, are due solely to the requirements or inducements of Latin idiom in the hands of the accomplished translator. A considerable body, however, of true 'Western' readings, not found in the Old Uncials or in the Antiochian text, remain, and their presence must be ascribed to the Old Latin text, or texts, in Jerome's hands, which he so carefully revised to create his great work.

That the Vulgate is not dependent on the Antiochian text would not be difficult to show.<sup>4</sup> Thus in chapters i and ii of Acts there are 25 readings (where the Latin is capable of showing the variation) in which the Antiochian text, as represented by a majority of the codices adduced for it in the apparatus of "The Text of Acts," departs from the Vaticanus. Five of these are cases where a majority (either three or four) of the Old Uncial witnesses have the same text as the Antiochian, so that the readings cannot be deemed specifically Antiochian. This leaves 20 Antiochian readings proper. In 13 instances out of the 20 the Vulgate does not agree with the Antiochian. The remaining seven readings in which the Vulgate agrees with the Antiochian text against a majority of the Old Uncial witnesses are all of insignificant importance. Two are slight differences of order; two consist in the use of  $\tau\epsilon$  for  $\delta\acute{\epsilon}$  or  $\kappa\alpha\iota$ ; and one of the others (the addition of  $\epsilon\phi\eta$  in 2, 38) closely resembles the reading ( $\phi\eta\sigma\iota\nu$ ) attested as 'Western' by Codex Bezae. It is not for a moment to be supposed that Jerome went for these trifles to an Antiochian codex which he might have had in his possession. And it is only a little less unlikely that they should have come to him by way of Antiochian influence on some Old Latin manuscript which he was following.

The fact is that whereas in the Greek minuscules (none earlier than the ninth century) Antiochian influence is to be expected and is generally easy to find, in any very early text on the contrary the question of such influence is very difficult

<sup>4</sup> For the different conclusion of Wordsworth and White, see below, p. 79. The point ought to be tested by someone operating with a larger body of material than the two chapters used in the present article.

and requires extremely careful handling. The discussion of the matter by any other than highly experienced and discreet students is usually unsatisfactory. In but small part, and that part, so far as we are aware, not as yet determined for any book of the New Testament, is the Antiochian text a new creation of readings which, wherever they occur, can be recognized as necessarily evincing an Antiochian source; in this circumstance the contrast of the Antiochian text to the 'Western' text is striking and important. The Antiochian text is rather, for the most part, a *specific selection* of readings previously existent; and the identity of the Antiochian text as a text depends on the possession of that particular selection, not on the presence of a few of these readings.

Furthermore, isolated agreement even in an unusual reading between the Antiochian text and an Old Uncial witness does not necessarily point to any direct relation between the two. The readings selected to form the Antiochian text came (by whatever actual process and on whatever principles of selection) from the Old Uncial text as main source, but also, as minor source, from the 'Western' text; and in any given case a 'Western' reading may be present in a single Old Uncial, or even in two, but yet have actually come into the Antiochian text, quite independently of the adoption of it by Old Uncial witnesses, as a part of the 'Western' ingredient of the Antiochian. Similarly with the versions: since most, if not all, of the oldest ones contain a 'Western' element, a case of agreement between a version and the Antiochian, especially in an unimportant detail, always needs to be looked at carefully to see whether it means anything more than the simple fact that both the version and the Antiochian have carried along numerous 'Western' survivals. The underlying process was usually, it would seem, the retention of inconspicuous 'Western' readings in a text which in the main the scribe intended to bring as nearly as was conveniently possible into accord with the standard (in a general way the Old Uncial) that had later become fashionable.

And finally, for the safe and successful pursuit of various lines of text-critical research in progress at the present day im-

portant desiderata are both the printing of a better Antiochian (or 'Byzantine') text than the mixed 'Textus Receptus' affords and also a careful study of the character, structure, origin, and motives of the Antiochian text itself.

The Vulgate, to return to that, is substantially the translation of an Old Uncial text, but in it a certain number of renderings of 'Western' readings (the proportion could be approximately determined by the aid of the tables in "The Text of Acts") have been preserved from the Old Latin. The form of the Old Latin used as the foundation for Jerome's revision had itself been revised to correspond to a non-western (probably Old Uncial) Greek text, and had already lost many of the 'Western' readings characteristic of the earliest Latin texts. For the Vulgate Jerome may have used either one or more than one Greek manuscript. In any case the codex or codices thus employed had a text of the general type of B  $\Sigma$  A C 81. The method of investigation must be to compare it with these MSS for all those Greek readings where the five are not in agreement. In doing this the body of material to be studied will be reduced to those readings where the Latin language is capable of rendering the distinction between the rival Greek readings. Automatically we thus elicit at the outset a particularly significant series of variants.

Such an examination brings out immediately two important facts. First, when Latin can represent Greek and when there is variation within the Old Uncial group of five, the Vulgate rendering agrees in by far the greater number of cases with that Greek reading which is attested by the majority of the five Greek codices. And secondly, the Greek codex that occupies the most nearly central position, and with which in consequence the Vulgate agrees most often, is Codex Alexandrinus, although Codex Vaticanus and Codex 81 stand near it in this respect. These two facts are complementary, and they are in accord with the observation <sup>5</sup> that A contains a relatively small

<sup>5</sup> See H. von Soden, *Die Schriften des Neuen Testaments*, vol. 1, pp. 877, 1662-1664, 1928; J. H. Ropes, *The Text of Acts*, pp. liii f., cclxviii.

number of readings which are 'singular' among the Old Uncials (although it has rather more of them than B or 81).

The figures which yield the former of these two results are as follows, the total number of readings which fall under consideration being about 524:

Vulgate agrees with	4 codices	358 times
" " "	3 "	81
" " "	2 "	32
" " "	B alone	12
" " "	8 "	10
" " "	A "	10
" " "	C "	13
" " "	81 "	8

---

Total ..... 524

It thus appears that in 84 per cent (439) of the 524 cases the Vulgate agrees with the majority (four or three) of the five Old Uncial codices. The 53 cases where it goes with the 'singular' reading of one codex against the other four are probably largely instances of 'Western' readings which have come into the Vulgate through the Old Latin, so that the isolated Old Uncial support here is without bearing on the problem. At all events, it is plain that the Vulgate bears no specific relation to the particular text of any one of the five codices.

One interesting fact brought out by these figures is what may be called the 'solidarity' of the Old Uncial text. In about 411 out of the 524 cases the variation is caused by the withdrawal of one Old Uncial witness from the reading in which the other four agree.<sup>6</sup> This is again just under four-fifths of the whole number, while in only about 113 cases, or just over one-fifth, do the five divide three against two. This frequent withdrawal of one codex from the consensus of the others has various causes. Often it is a mere error or vagary of the individual codex. In other instances it probably represents a special 'Western' or (especially in the late Codex 81) Antiochian element in the text of the dissentient manuscript. But it is plain

<sup>6</sup> The cases in this series of variations where the five Old Uncial witnesses present three different readings are so few as to be negligible, and they have in fact been neglected in summarizing the conclusions.



that in the matter of readings sufficiently important to be capable of distinct representation in a Latin rendering the text of this family maintained for several centuries a surprising uniformity and did not split up into permanent minor or secondary types on any large scale. This observation tends to confirm confidence in the fidelity of our best Old Uncial witnesses to the fundamental text which they represent, and is not without its bearing, so far as Acts is concerned, on Westcott and Hort's hypothesis of an "Alexandrian" text which is supposed to have been a revision of the "Neutral" text of B and  $\aleph$ . Since in Acts B and  $\aleph$  do not agree against the others often enough to permit their common text to be regarded as a proper "Neutral" text distinct from an "Alexandrian" revision,<sup>7</sup> it is natural to ascribe their 'singular' readings in the series under consideration (B, 63 'singular' readings;  $\aleph$ , 100), as well as their common readings against the other three codices (17 readings only), like those of the other codices, largely to individual error, but partly also to special influences (good and otherwise) which have affected their ancestry.

Whatever may be thought of these matters, it is evident that the Vulgate revision was made by the aid of a Greek text which usually followed the consensus of MSS of the Old Uncial type. Jerome may have had a single excellent Greek copy which he followed, or he may have avoided the eccentricities of any one copy by following the consensus of several good copies in his possession. Since the readings he followed seem to bear no specific relation to any one of our extant witnesses, those of them (about one-fifth) in which he follows a minority of our five codices deserve to be examined individually to see whether they may represent the ancient consensus (and perhaps the true text) better than do the few most trustworthy copies which happen to be accessible to us.<sup>8</sup> But the claim sometimes made, that Jerome's Vulgate, as it stands, is to be taken as the basis of New Testament textual criticism because he worked

<sup>7</sup> The Text of Acts, pp. cclxi f.; cf. pp. cclxxii-cclxxiv.

<sup>8</sup> This is one of the many examples of studies that must be made before it is attempted to construct a fresh critical Greek text of the New Testament. That task involves more kinds of preliminary and incidental research than is sometimes imagined.

with Greek mss that were in no case written later than the fourth century and was himself a most accomplished scholar, is not supported by what we can infer as to his method of work. The Greek text he followed was an excellent one, but in any individual case it may be wrong; and moreover he sometimes retained, with or without slight modification, Old Latin renderings of a different and inferior Greek text.

As to the second of the two results mentioned above, the position of Codex Alexandrinus, the distribution of Vulgate readings among the several Old Uncial witnesses in our series of 524 readings is as follows:

Vulgate agrees with	A	388 times	74%
" " " B	371	"	71%
" " " 81	361	"	69%
" " " N	335	"	64%
" " " C	334	"	64%

These figures reflect the same characteristics of N and C, as noteworthy for abundance of 'singular' readings, which other investigations have shown,<sup>9</sup> but they also, rather unexpectedly,<sup>10</sup> bring to light the fact that in serious variants (and in the nature of the case these alone come in question in the present study) A goes with the majority of the Old Uncials more often than does any other of the five. This can be shown more directly by the following table for our 524 readings. Here, as elsewhere, certain minor complications are neglected, so that the figures are to be taken as approximate, but the broad bearing of the statistics is trustworthy.

A agrees with majority of five codices	423 times	81%
81 " " " " " "	409	" 78%
B " " " " " "	408	" 78%
N " " " " " "	388	" 74%
C " " " " " "	355	" 68%

<sup>9</sup> The Text of Acts, pp. cclvii f., cclxvi, cclxix.

<sup>10</sup> That this fact was not brought out by the examination of 'singular' readings in "The Text of Acts" probably indicates that in small variants (including the use of the article), where the Latin makes no distinction, A has a larger proportion of aberrant readings than either B or 81. These small variants were included in the figures used in "The Text of Acts," but not in the tabulations of the present article. If we consider all kinds of 'singular' readings in those portions of Acts where all five witnesses are extant, the figures are: B, 96; Codex 81, 101; A, 120; N, 158; C, 186.

The superiority of A over 81 and B in representing the consensus is not large, but it is distinct.

The relation of the Vulgate to Codex Alexandrinus in Acts, of which the real ground seems thus satisfactorily explained, recalls the statement of Hort ('Introduction,' p. 152) that the text of A "in several books" is much like the Greek text by which the Vulgate was revised.

Wordsworth and White ('Epilogus' to the Four Gospels, p. 660; 'Praefatio in Actus,' p. x) were convinced that the Greek text used by Jerome in the gospels was largely similar to that of the Codex Sinaiticus. They observed ('Praefatio in Actus,' pp. xii f.), as we have done, that Jerome generally followed the majority against a single dissident, but do not seem to have noted the peculiar relation to Codex Alexandrinus into which this brings his text in Acts. Their observation (p. x, note 2) that Jerome in Acts follows the Antiochian readings of H L P one-half as often as the readings of B  $\aleph$  A C is probably better explained by the considerable number of 'Western' readings which enter into the Antiochian text of H L P than by their conclusion that Jerome had at hand Antiochian Greek manuscripts as well as those of Alexandrian type.<sup>11</sup>

One further line of investigation of the figures can be readily pursued, but does not seem to lead to clear or important conclusions. Where one Old Uncial witness departs from the other four (as, for instance, in the cases of B against  $\aleph$  A C 81), it is easy to calculate the proportion of cases in which the Vulgate follows the one witness to those in which it departs from the one and follows the majority. The result of such a reckoning is to show that there is a slightly greater inclination of the Vul-

<sup>11</sup> Of the fifteen instances of agreement with H L P adduced by Wordsworth and White ('Praefatio in Actus,' p. xii, note 2, concluding paragraph) one (Acts 26, 28 *feri*) is a case where the 'Western' reading is not known, while of the others all but four (16, 6 *transeuntes*; 19, 37 *vestram*; 20, 25 *dei*; 24, 21 *in me*) are readily explained as 'Western' survivals in the Vulgate and the Antiochian alike but independently; and as to the four exceptions just named (no one of which can be called an important or striking reading) there is in every instance something to be said against the necessity of assuming direct Antiochian influence. One of the most noteworthy of the fifteen (15, 18) is, in the light of all the facts, very distinctly a case where the reading is not due to an Antiochian ms. As has been remarked above, a perfectly satisfactory conclusion would require an examination of the whole body of material.

gate to agree with B standing alone than with any other one of the five standing by itself. But, especially in view of the possible margin of error in the figures, the numbers are too small, and the difference in the percentages too slight, to permit any general conclusion to be drawn with much confidence. For the comparative percentages on this point for the four versions, see below, Table C, p. 92.

In conclusion the figures are appended on which the above discussion is based; it must again be remarked that they are to be taken as approximate only.

Vulgate agrees with

§ A C 81 .....	52 times
B A C 81 .....	90
B § C 81 .....	60
B § A 81 .....	93
B § A C .....	63
<hr/>	
B .....	12
§ .....	10
A .....	10
C .....	13
81 .....	8
<hr/>	
A C 81 .....	14
§ C 81 .....	4
§ A 81 .....	13
§ A C .....	9
B C 81 .....	2
B A 81 .....	8
B A C .....	6
B § 81 .....	8
B § C .....	2
B § A .....	15
<hr/>	
B § .....	3
B A .....	3
B C .....	3
B 81 .....	3
§ A .....	2
§ C .....	1
A C .....	8
A 81 .....	2
C 81 .....	7
<hr/>	
Total .....	524

## II

## ORIENTAL VERSIONS

In the Oriental versions statistics showing the relation of the versions to the several Greek mss are in the nature of the case far more laborious to make, and somewhat less satisfactory in result, than for the Vulgate. The great difference in structure and expression between all these languages and Greek makes a decision difficult as to where the Oriental language permits us to infer the underlying Greek reading, and the judgment of different scholars is sure to differ. Moreover, in the Sahidic and Bohairic translations questions of the text of the version itself are pressing, and opinions are diverse. Where the numbers themselves are no larger than those with which we are here dealing, such uncertainties gravely affect the significance of the statistics. In the present inquiry it has not been possible to include more than about one half of that portion of Acts in which all five Old Uncial witnesses are extant, namely, Acts i. 2-iv. 3; vii. 17-x. 42. The examination of the other portions would probably not throw additional light on the questions raised.

1. *Peshitto*.<sup>12</sup> In Acts, where the textual relations of the Peshitto seem to be different from those which it shows in the gospels, this version contains many 'Western' readings, but its text is mainly that of the Old Uncial family. As suggested above, there are several ways in which the 'Western' readings may have come in. They may be survivals of an Old Syriac base, itself made, perhaps as early as the second century, from a 'Western' Greek text. Or they may have come from a 'Western' element in a mixed Greek manuscript used as a guide by the Syriac revisers. Or, thirdly, the Syriac revisers may have had at hand Greek manuscripts of more than one type and have drawn their readings now from one and now from another of these. And it is further possible that two or even all three of these causes coöperated to produce the actual

<sup>12</sup> The text used for collating was the edition of the British and Foreign Bible Society, 1920.



result. The simplest hypothesis, recommended by the analogy of the Latin Vulgate, would seem to be the first of the three, namely, that the 'Western' readings are due to the Old Syriac base, while the general Old Uncial character came from the Greek MS or MSS used in the revision: but the question cannot be discussed here. Whether the proportion of 'Western' readings in the Peshitto Acts is greater or less than in the Vulgate Acts could be approximately determined by the use of the tables in Appendix III of "The Text of Acts," coupled with a further study to elicit the renderings which not only are consistent with but positively imply non-western Greek readings; but it is not likely that the result would throw any great light on the problems either of textual criticism or of textual history. At all events, the 'Western' readings attested by the Peshitto are considerable in number and often noteworthy in character.

As to possible relation to the Antiochian text, there is no reason why the Syriac revisers, who made the Peshitto early in the fifth century, should not have had at their disposal a copy of that text, since in their day it had probably been in existence for a century or more; and moreover the generally admitted Antiochian character of the Peshitto text of the gospels might suggest that in Acts also they would follow the same standard. But the facts seem to show that they did not do so. Unfortunately it has not been possible for us to collect statistics covering a sufficiently large number of chapters to give numerical proof of this. In Acts, chapters i and ii, which we have examined, most of the Antiochian variants from the Old Uncial text are not easily susceptible of a distinctive Syriac rendering, and the remainder are too few to provide significant results. Yet from such study as we have been able to make, and still more from the exhibit of variants from B in the Peshitto Tables of "The Text of Acts," it is evident that many of the very moderate number of agreements of the Peshitto with the Antiochian are probably to be explained as parallel 'Western' survivals in both texts independently; and of the rest a large proportion are readings of trifling significance. The characteristic selection of substantial variants which makes up the recognizable Antiochian text of Acts does not appear in any domi-

nant fashion in the renderings of the Peshitto. This situation could hardly be present if the Antiochian text had been an important guide for the work of the Syriac revisers.

A highly noteworthy trait of the Peshitto in Acts is the great freedom permitted themselves by the translators, a freedom often resembling in method that of the 'Western' revisers of the Greek text, though quite independent of the latter in its actual application. In this trait the Peshitto stands at the opposite pole to the skilful literalness of the Latin Vulgate.

When we come to a study of the relation of the Peshitto to our five chief Old Uncial witnesses, the situation is in some respects not very different from that of the Vulgate. In the parts of Acts examined for this purpose we have found 178 cases in which the Peshitto definitely follows one rather than the other of the Greek variants within the Old Uncial family. As would be expected, it generally agrees with the majority (four or three) of the five, namely in 133, or 75 per cent, of the 178 cases (against 84 per cent for the Vulgate). In the matter of agreement with 'singular' readings of the five, the numbers, though small, are curiously like those for the Vulgate, and the order of the several witnesses is practically the same (C, B and **Σ**, A and 81; compared with C, B, **Σ** and A, 81, in the case of the Vulgate). The figures are as follows:

Peshitto agrees with	4 codices	107 times
" " "	3 "	26 "
" " "	2 "	17 "
" " "	B alone	6 "
" " "	<b>Σ</b> "	6 "
" " "	A "	3 "
" " "	C "	10 "
" " "	81 "	3 "
		— "
Total .....		178 "

In this series of 178 readings, it may be remarked in passing, the five Old Uncial witnesses divide four against one 135 times (76 per cent). The corresponding figure for the Vulgate series was 78 per cent. The two series of Greek variants, as would be expected, have the same quality in this respect.

The relation of the Peshitto to the individual Greek MSS is exhibited by the following table:

Peshitto agrees with	B	126 times	71 %
" " " 81	118	"	66 %
" " " C	117	"	66 %
" " " A	112	"	63 %
" " " N	95	"	53 %

The difference of these percentages from those for the Vulgate is striking. While the degree of agreement with B is exactly the same, the preëminence we there found for A entirely disappears, and N falls with a decisive drop to the bottom of the list. The figures by themselves do not suggest to us any convincing explanation, but it would look as if the type of Old Uncial text which appears in the Peshitto were somewhat different from the type (doubtless of Alexandrian source) followed by Jerome. Yet the complication due to the different infusions of 'Western' elements into the two versions must cause the question to remain obscure. And the numerical differences between the several items (except in the case of N, on which something will be said in the next paragraph) are not very large.

That the situation here is not wholly due to a chance difference in the character of the series of readings used for the Peshitto from that used for the Vulgate is shown by the following table:<sup>13</sup>

A agrees with majority of five codices	145 times	81 %
81 " " " " " "	141	" 80 %
B " " " " " "	139	" 80 %
C " " " " " "	124	" 70 %
N " " " " " "	120	" 67 %

This table shows that the degree of agreement of A with the majority is no different, and that of 81, B, and C not significantly different, from what is found in the Vulgate series of readings (see above, p. 78). For some unknown reason N in this series agrees less often with the majority than in the Vul-

<sup>13</sup> For a comparison of similar percentages for all four versions, including the Sahidic and Bohairic, see Table F below, p. 93.

gate series, but this difference is not great enough to account for the whole of the drop in the percentage of agreement of the Peshitto with **N** brought out in the table on page 84.

The fact (see Table C, p. 92, below) that the Peshitto proportionately agrees much more often with **B** against the group **N A C 81** (40 per cent in 15 cases of this particular division into groups) than does the Vulgate (19 per cent in 64 cases), suggests that there is some real relation between the Peshitto and the text of **B**. Likewise (in the same Table C) the proportion of agreements of the Peshitto with **C** against **B N A 81** (31 per cent in 32 cases) is noteworthy, and perhaps points to some real relation of text. But in both observations the absolute numbers are very small, and in such cases a few instances of difference in judgment as to the validity of the evidence of the version might make a large difference in the percentage.

The figures for the 178 cases on which this discussion of the Peshitto is based are as follows:

Peshitto agrees with

<b>N A C 81</b> .....	9 times
<b>B A C 81</b> .....	35 "
<b>B N C 81</b> .....	20 "
<b>B N A 81</b> .....	22 "
<b>B N A C</b> .....	21 "
<hr/>	
<b>B</b> .....	6 "
<b>N</b> .....	6 "
<b>A</b> .....	3 "
<b>C</b> .....	10 "
<b>81</b> .....	3 "
<hr/>	
<b>A C 81</b> .....	6 "
<b>N C 81</b> .....	2 "
<b>N A 81</b> .....	6 "
<b>B C 81</b> .....	3 "
<b>B A 81</b> .....	2 "
<b>B A C</b> .....	2 "
<b>B N 81</b> .....	2 "
<b>B N A</b> .....	3 "
<hr/>	
<b>B N</b> .....	1 "
<b>B A</b> .....	1 "

B C .....	3 times
B 81 .....	5 "
Ⲛ C .....	2 "
Ⲛ 81 .....	1 "
A C .....	2 "
C 81 .....	2 "
<hr/>	
Total .....	178

2. *Sahidic*.<sup>14</sup> The Sahidic version seems to have been made from a Greek text which had been mainly conformed to an Old Uncial standard, but in which some 'Western' survivals persisted, these being chiefly variants of trifling importance. The version was made hardly later, and perhaps considerably earlier, than the Antiochian text, and the consequent presumption that it will show no Antiochian influence is fully confirmed by an examination of the facts. In the relatively few cases where the Sahidic agrees with the Antiochian text, this agreement may confidently be ascribed to coincidence, to 'Western' influence working upon the two independently, or to a slightly different form of the Old Uncial type from that determinable from our manuscripts.

In considering the relation of the Sahidic to the five Old Uncial witnesses, we follow the same method as for the Vulgate and the Peshitto. The total number of cases available for our use where variation occurs within the Old Uncial family is 206.

Sahidic agrees with	4 codices	127 times
" " "	3 "	28 "
" " "	2 "	23 "
" " "	B alone	11 "
" " "	Ⲛ "	5 "
" " "	A "	3 "
" " "	C "	6 "
" " "	81 "	3 "

Of the 206 cases the Sahidic agrees with the majority (four or three) 155 times, or 75 per cent.

<sup>14</sup> Horner's text, Oxford, 1922, has been used for collation.



The agreement with the several manuscripts is as follows:

Sahidic agrees with	B	160 times	78%
" " "	81	139 "	67%
" " "	A	132 "	64%
" " "	C	120 "	58%
" " "	Σ	111 "	54%

This shows an even higher degree of agreement with B than in the Peshitto, and the same small degree of agreement with Σ, while C, unlike its situation in the Peshitto, falls almost to the low position of Σ. The relation to A is the same as in the Peshitto, and again there is no trace of the Vulgate preëminence of this codex. The enhanced agreement with B is accented, although the absolute numbers are small, by the fact that, in the 23 cases where B stands against the group Σ A C 81, the Sahidic agrees with B in 11 instances, or 48 per cent. This fact is made all the more striking by the low proportion of agreement (ranging from 11 per cent to 16 per cent; see Table C, below, p. 92) which the Sahidic shows for every one of the other four Old Uncial manuscripts. The proportion of agreement with B is larger, as will be shown presently, than is furnished even by the Bohairic version. The presence, however, in the Sahidic of a considerable (though for the most part intrinsically uninteresting) 'Western' element, absent in the Bohairic, makes its text as a whole less close to B than is the Bohairic.

The figures for the 206 cases of variation in our Sahidic series are as follows:

Sahidic agrees with

Σ A C 81 .....	12 times
B A C 81 .....	42 "
B Σ C 81 .....	20 "
B Σ A 81 .....	32 "
B Σ A C .....	21 "
<hr/>	
B .....	11 "
Σ .....	5 "
A .....	3 "
C .....	6 "
81 .....	3 "

A C 81 .....	4 times
ⲁ C 81 .....	2 "
ⲁ A 81 .....	4 "
ⲁ A C .....	2 "
B C 81 .....	2 "
B A 81 .....	4 "
B A C .....	3 "
B ⲁ 81 .....	5 "
B ⲁ A .....	2 "
<hr/>	
B ⲁ .....	6 "
B A .....	1 "
B C .....	4 "
B 81 .....	7 "
A C .....	1 "
A 81 .....	1 "
C 81 .....	3 "
<hr/>	
Total .....	206

3. *Bohairic*.<sup>15</sup> The Bohairic version, as is well known, is to be classed with the Old Uncial witnesses, and does not seem to include any considerable 'Western' element. Although the Bohairic translation was probably made not earlier than the seventh century, the Antiochian text seems not to have affected the Greek manuscript used. In the Bohairic, as in the Sahidic, it is not always easy to decide in small matters, such as the order of words and the presence or absence of the article, whether the Bohairic rendering is due to the idiom of that language or to a Greek variant to which, superficially at least, it corresponds. The collation used in the present article was made with rather more generous acceptance of the Bohairic evidence for Greek variants than was allowed in Thompson's collation, on which the Bohairic tables in Appendix V of "The Text of Acts" are founded. This divergence of judgment will account for some inconsistencies, but probably does not greatly affect the general result.

The collation here used yields a series of 203 cases where a Greek variant appears to the collator to be reflected in the Bohairic.

<sup>15</sup> Horner's text, Oxford, 1905, has been used for the collation.

The proportion in which the Bohairic agrees with the majority (four or three) of the Old Uncial manuscripts is 79 per cent (161 out of the 203 cases).

Bohairic agrees with	4	codices	125	times
" " "	3	"	36	"
" " "	2	"	15	"
" " "	B	alone	10	"
" " "	Ⲛ	"	4	"
" " "	A	"	1	"
" " "	C	"	6	"
" " "	81	"	6	"

---

203

The figures for agreement with individual codices are:

Bohairic agrees with	B	147	times	72%
" " "	81	144	"	71%
" " "	A	135	"	67%
" " "	C	126	"	62%
" " "	Ⲛ	113	"	56%

The surprising fact here is that the figures show a less proportion of agreement with B than was the case with the Sahidic (78 per cent), which on the whole (as is shown by the tables in "The Text of Acts") departs from B much more than does the Bohairic. There is no contradiction, however. The Bohairic has its close relation to B because it is not led to diverge by any such 'Western' strain within it as has entered into the Sahidic. Where, however, as in the figures just given, it is merely a question of correspondence with B *as compared with the other Old Uncials*, the Sahidic seems to come closer to B than the Bohairic. There are in Acts some striking instances of agreement of the Bohairic with B against all other witnesses whatsoever which should be a warning against putting too great reliance on general statements founded on purely statistical evidence rather than on the internal character of particular circumstances.

Of the cases where B stands alone against Ⲛ A C 81, the Bohairic agrees with B in 45 per cent (10 times out of 22), a

little less often than the Sahidic. Where 81 stands against B & A C, the Bohairic agrees with it in 25 per cent of the 24 cases, an unusually high proportion. The relation of the Bohairic to 81 has probably some significance; at least it points anew to the excellence of that Greek manuscript, late (A.D. 1040) as it is.

The figures for the 203 cases of variation in our Bohairic series are as follows:

Bohairic agrees with

ⲛ ⲁ Ⲙ 81 .....	12	times
B ⲁ Ⲙ 81 .....	40	"
B ⲛ Ⲙ 81 .....	22	"
B ⲛ ⲁ 81 .....	33	"
B ⲛ ⲁ Ⲙ .....	18	"
<hr/>		
B .....	10	"
ⲛ .....	4	"
A .....	1	"
C .....	6	"
81 .....	6	"
<hr/>		
A Ⲙ 81 .....	8	"
ⲛ Ⲙ 81 .....	2	"
ⲛ ⲁ 81 .....	8	"
ⲛ ⲁ Ⲙ .....	5	"
B Ⲙ 81 .....	3	"
B ⲁ 81 .....	2	"
B ⲁ Ⲙ .....	3	"
B ⲛ 81 .....	4	"
B ⲛ ⲁ .....	1	"
<hr/>		
B ⲛ .....	3	"
B ⲁ .....	1	"
B Ⲙ .....	4	"
B 81 .....	3	"
ⲛ Ⲙ .....	1	"
A Ⲙ .....	2	"
A 81 .....	1	"
<hr/>		
Total .....	203	

## III

The following comparative tables present certain facts for the four versions, and bring out clearly some aspects of the investigation.

TABLE A

Order in which the codices stand in respect of their agreement with the several versions:

Vulgate .....	A	B	81	Ⲣ	C
Peshitto .....	B	81	C	A	Ⲣ
Sahidic .....	B	81	A	C	Ⲣ
Bohairic .....	B	81	A	C	Ⲣ

TABLE B

The order of agreement for the several codices given in Table A is founded on figures which, when reduced to percentages of the total number of cases in the series for each version, yield the following table:

				Vulgate %	Peshitto %	Sahidic %	Bohairic %
Version agrees with	B			71	71	78	72
"	"	"	Ⲣ	64	53	54	56
"	"	"	A	74	63	64	67
"	"	"	C	64	66	58	62
"	"	"	81	69	66	67	71

The most conspicuous points observable here are the high figures for agreement with B in the Sahidic, and for A in the Vulgate; together with the fact that in every case, even including the Vulgate (where Ⲣ does rise above C by one instance — 335 as against 334), Ⲣ stands virtually at the bottom of the list. The close approximation of 81 to B in the Vulgate and the Bohairic is also noticeable.

TABLE C

When the Old Uncials divide four against one, the several versions agree with the one dissident manuscript in the following percentages, the total number of cases of variation by that particular four-to-one division being taken as 100 per cent in



each column. The absolute numbers can be learned from the general lists of groups for each version given above in the successive sections of this article.

Out of all cases where the respective codex stands alone, the version agrees with B alone . . .	Vulgate %	Peshitto %	Sahidic %	Bohairic %
" " " <b>N</b> " . . .	19	40	48	45
" " " <b>A</b> " . . .	10	15	11	9
" " " <b>C</b> " . . .	14	13	16	4
" " " <b>81</b> " . . .	12	31	16	15
" " " <b>81</b> " . . .	11	13	13	25

TABLE D

As a kind of appendix to Table C the corresponding figures for Codex B when it is supported by only one other of the five Old Uncial witnesses may be given:

Vulgate agrees with B and one other	12 times out of 40	30%
Peshitto " " " " " "	10 " " " 26	39%
Sahidic " " " " " "	18 " " " 30	60%
Bohairic " " " " " "	11 " " " 34	32%

With reference to both Table C and Table D the warning must again be emphasized that the absolute numbers which yield the percentages are often too small to be fully significant, and that in the Sahidic, and (especially for our figures) the Bohairic, some difference of judgment is possible as to which renderings give valid evidence of Greek readings. But it is possible that the line of inquiry here suggested will sometime prove instructive when the history of the Old Uncial text is worked out in detail for the ten centuries (the fourth century to the thirteenth) in which we know from extant mss that it was used. The Sahidic probably represents an older form of the Old Uncial type than does the Bohairic.

TABLE E

The degree to which the several versions tend to agree with the majority of the five Old Uncial witnesses, as against aberrant 'singular' Greek readings or against readings in which

only three or two Greek witnesses concur, is exhibited in the following table:

	Vulgate	Peshitto	Sabidic	Bohairic
Total number of cases of variation comprised in each series . . . . .	524	178	206	203
	%	%	%	%
Version agrees with 4 codices . . . . .	68	60	62	61
“ “ “ 3 “ . . . . .	15	15	13	17
“ “ “ 2 “ . . . . .	6	9	11	7
“ “ “ 1 codex . . . . .	10	16	13	13

The tendency to agree with the consensus of four codices is much stronger in the Vulgate than in the oriental versions.

TABLE F

In order to test the character of the four series of variations used for the several versions in the present article, the following figures (already given above for the Vulgate and Peshitto, in connection with the discussion of those versions) are combined in a comparative table. This table is not supposed to throw any light on the character of the several codices; as an investigation of the codices it would be wholly inadequate, since the percentages are based in each case merely on a selected series comprising but a part of the Greek variants. Its purpose is rather to discover whether or not the principle of selection (namely, capability of being represented in the various languages) elicits in each series a body of readings free from any distinctive character, or whether, by virtue of the method of selection, some kind of systematic error is introduced into the result. On the whole the result is favorable, though some unaccountable facts emerge.

	Vulgate series	Peshitto series	Sabidic series	Bohairic series
Total number of cases in series . . . . .	524	178	206	203
	%	%	%	%
B agrees with majority of 5 codices . .	78	80	74	71
N “ “ “ “ “ “ . .	74	67	68	68
A “ “ “ “ “ “ . .	81	81	83	81
C “ “ “ “ “ “ . .	68	70	70	69
81 “ “ “ “ “ “ . .	78	80	80	81

The perplexing fact here is that in the Vulgate series Codices B and N, and in the Peshitto series Codex B, show a marked excess of agreement with the majority above that which they show in the other versions, while everywhere else all the numbers present the general uniformity which would be expected. The Latin language is of course capable of representing a much greater number of Greek variants than are the three oriental languages; indeed, nearly all Greek variants except the varying use of the article and some differences in grammatical form are expressible in Latin. In the section of Acts examined for the Vulgate, in 70 per cent of the cases of variation noted in the Old Uncial apparatus of "The Text of Acts" (524 cases out of about 748) it is possible to say which Greek variant is represented by the Latin rendering, and the similarity of Latin to Greek may have something to do with the point in question. It is also true that Syriac is a much more highly developed language than the Egyptian dialects. But why these distinctions should apply only to some Greek mss and not to the others passes our ingenuity to suggest. The question may be stated thus: Why should the criterion of translatability into Latin select a larger proportion of readings in which Codex Vaticanus stands with the majority of the five codices than does the criterion of translatability into Bohairic?

In closing, a few conclusions and observations, for the most part already stated above in the article, may be summed up. It is to be remarked at the outset that the body of material here examined is in the nature of the case limited to variants of some importance; least so in the Vulgate, most so in the two Egyptian versions.

1. The strong tendency of every one of the versions to follow the majority of the Old Uncial witnesses, and especially not to agree with 'singular' readings of any one codex, is very noteworthy, and tends to increase the suspicion in which on other grounds such 'singular' readings are held.

2. The prominence of the readings of Codex Vaticanus is marked in all the versions, although least in the Vulgate, where B is surpassed by Codex Alexandrinus.

3. The close relation of Codex 81 to B is brought out by the comparisons, but the 'singular' readings of 81 (except in the Bohairic) appear proportionately rather less often in the versions than do those of A and C, and with about the same frequency as those of N.

4. The closeness of the Sahidic to B as compared with the other Old Uncials, and in so far as the Sahidic readings are not 'Western,' is an unexpected fact elicited by the study. The reason why the Sahidic is closer to B than is the Bohairic in the cases of variation here examined, while less close to it on the whole (because of the 'Western' element in the Sahidic), has been discussed above. It seems not improbable that the Sahidic represents an earlier form of the Old Uncial type than does the Bohairic.

5. The striking preëminence of the readings of Codex Alexandrinus in the Vulgate and the reason shown for this are important, and may well tend to inspire a greater respect for the quality of that manuscript, which seems in Acts to contain a particularly good average Old Uncial text. Especially, this quality of A in Acts seems to lend force to the suggestion that in the gospels the text of A may give a relatively pure text of the oldest form of the Antiochian type, instead of offering, as is often supposed, a highly mixed text. This question, raised by Professor Lake, deserves a fresh examination.

6. The general unpopularity with all the versions of the readings of Codex Sinaiticus (see Table B) is remarkable, and is only partly explained by the relatively large number of unsatisfactory 'singular' readings in that codex.

7. The relations of Codex C are uncertain, as indeed the text and history of that Great Bible remain in general hitherto unexplained.

Last of all it may be repeated that the study of these figures has impressed upon us anew the extreme difficulty of the use of statistics in textual criticism. Only where the question to be answered is of great simplicity are statistics likely to be conclusive. Elsewhere the greatest wariness and delicacy in handling them is requisite.





# THE TEXT OF THE ANTI-MANICHAEAN WRITINGS OF TITUS OF BOSTRA AND SERAPION OF THMUIS

ROBERT PIERCE CASEY

UNIVERSITY OF CINCINNATI

In the catalogue of manuscripts at the Monastery of Vatopedi on Mount Athos by Eustratiades and Arcadios there is a curious error in the description of No. 236.<sup>1</sup> The manuscript contains a variety<sup>2</sup> of patristic treatises, letters, and fragments, four of which are thus described:

- 15. φ. 38<sup>a</sup> Σεραπίωνος ἐπισκόπου Θμουέως κατὰ Μανιχαίων.
- 16. φ. 59<sup>β</sup> Τοῦ αὐτοῦ κατὰ Μανιχαίων.
- 17. φ. 71<sup>β</sup> Τοῦ αὐτοῦ λόγος β'.
- 18. φ. 89<sup>β</sup> Τοῦ αὐτοῦ λόγος γ'.

This notice might well rouse either the hopes or the suspicions of an investigator, for the treatise of Serapion against the Manichees, printed in Migne, P. G., XL, 900–924, contains only the fragmentary remains of a single book, and there is no evidence that more than one book ever existed. An examination of the manuscript shows that suspicion was justified, and that in the second item, No. 16 on f. 59 b, τοῦ Τίτου should be read for τοῦ αὐτοῦ. Nos. 16, 17, and 18 are the first three books of Titus of Bostra, 'Against the Manichees.'<sup>3</sup>

<sup>1</sup> Catalogue of the Greek Manuscripts in the Library of the Monastery of Vatopedi on Mt. Athos, by Sophronios Eustratiades and Arcadios (Harvard Theological Studies XI), Cambridge, Mass., 1924.

<sup>2</sup> Several other anti-manichaean writings are found in this manuscript, including one by John the Grammarian not mentioned by Krumbacher, *Geschichte der byzantinischen Literatur*, 2nd ed., p. 56.

<sup>3</sup> I have used the photographs of the manuscript from the J. Pierpont Morgan Collection of manuscript photographs belonging to the Harvard Library. Unfortunately the negative of folia 59b–60a, containing the end of Serapion's treatise and the beginning of Titus, book i, was imperfect, and I am unable to remember the exact form of the superscription.

## I

## MANUSCRIPT SOURCES FOR THE TEXT

THE text of the anti-manichaeon writings of Serapion and Titus has had a complicated history. The first edition of both appeared in Canisius' *Antiquae Lectiones*, in a Latin translation made by the Spanish Jesuit, Franz de Torres.<sup>4</sup> A revised and enlarged edition of this work was prepared by Jacob Basnage,<sup>5</sup> who heard that the Greek text from which Torres' translation had been made was in a manuscript in Lukas Holste's library at Hamburg. In his preface to Serapion's treatise (p. 37) he writes:

Unus superest Serapionis Libellus adversus Manichaeos. Hunc Latinum ediderat Canisius: sed monuit me vir eruditissimus Fabritius, Graecum exemplar, quo usus erat Turrianus Interpres, haberi in Bibliotheca Holsteniana, propositumque Professori celeberrimo apud Hamburgenses, D. Winklero, illud publici iuris facere. Scripsi ad Winklerum; postulavi, ut si quae jam edita haberet, mecum communicare vellet. Ipse summa cum humanitate non modo exemplar Graecum Serapionis, sed etiam Titi Bostrensis a se descriptum transmisit, ut in Collectione Canisiana ederentur. Debentur igitur viro doctissimo Graeca, quae nunc versioni Latinae adjicimus. Utinam notas, quas in geminos illos Scriptores meditabatur, eodem tempore mandari curasset! sed variis momentosissimisque curis distracto illas jam accumulatas multo labore et studio, in ordinem redigere et adornare non licuit.

This transcript which Winkler sent to Basnage has been the basis of all printed Greek texts of Serapion, and, until Lagarde's work on Titus, was likewise the only source of knowledge for the text of Titus.

In spite of its usefulness in leading Basnage to a Greek text of Serapion and Titus, Fabricius' information that Holste's manuscript was the basis of Torres' translation appears to have been wrong. In Migne, P. G., XVIII, 1156.2 (Lagarde, p. 38. 11) <sup>6</sup> the text of H <sup>7</sup> reads: *ἐὶ δὲ τις εὔροι ἂν τοὺς μὲν ἀπαθεῖς*

<sup>4</sup> H. Canisius, *Antiquae Lectiones*, vol. V, Ingolstadt, 1608.

<sup>5</sup> J. Basnage, *Thesaurus monumentorum ecclesiasticorum et historicorum sive Henrici Canisii Lectiones antiquae ad saeculorum ordinem digestae*, etc., vol. I, Amsterdam, 1775.

<sup>6</sup> References to Lagarde's Greek text are to Titi Bostreni quae ex opere *Contra Manichaeos* edito in codice Hamburgensi servata sunt graece e recognitione Pauli Antonii de Lagarde, Berlin, 1859. References to his Syriac text are to Titi Bostreni *Contra Manichaeos libri quatuor syriace*, Berlin, 1859.

<sup>7</sup> The following symbols are used for the manuscripts of Serapion and Titus:

φύσει τῶν ἀνθρώπων, τοὺς δὲ ἐμπαθεῖς καθ' ἐκάτερον, ψυχὴν τε καὶ σῶμα, καὶ τοὺς μὲν ἄνευ τροφῆς ἢ τοὺς μὲν ἀνεπιδέκτους κακίας, τοὺς δὲ δεκτικούς, ἔτι μὴν τοὺς μὲν θνητοὺς, τοὺς δὲ ἀθανάτους, καλῶς ἂν ὁ τοιοῦτος ἀταξίαν τε καὶ ἀνισότητα κατείποι τῆς διοικήσεως, ὡς ἀποκληρώσει τινὶ μᾶλλον ἢ λόγῳ τοῦ θεοῦ τὰ καθ' ἡμᾶς διοικούντος.

It is evident that a clause is needed to balance καὶ τοὺς μὲν ἄνευ τροφῆς, and Basnage suggested that τοὺς δὲ μετὰ τροφῆς be added.<sup>8</sup> The true text is supplied by V G which after ἄνευ τροφῆς add διαζῶντας, τοὺς δὲ διὰ τροφῆς with which Torres' Latin agrees; *si quis reperire posset alios quidem homines natura ab affectionibus animae et corporis liberos, alios vero non liberos; rursus alios sine cibo viventes, alios cibo alentes vitam*, etc. Similarly in Migne, P. G., XVIII, 1137.11 (Lagarde, p. 28.22-23) H reads, πῶς ἂν οὖν γένοιτο ἄνθρωπος ἀγαθὸς ζητητέον, εἰ πεποίηκεν αὐτόν. After ζητητέον, V G add καὶ οὕτως τὸν θεὸν ἀπαιτητέον, with which Torres' Latin agrees, *quomodo autem fiat homo probus, inquirendum est atque requirendum a Deo, utrum fecerit eum*. In Migne, XVIII, 1136.22 (Lagarde, p. 27, 29-30) the incomplete sentence, ἡ τοίνυν ἐξουσία τῶν ἐναντίων τὴν κτῆσιν is given from H. Comparison with V G shows that before τὴν κτῆσιν the words ποιεῖ τῶν ἐναντίων had dropped out, but Torres'

H. Hamburg, Stadtbibliothek, Philosophi gr. in-fol. xvi, saec. xvii; H. Omont 'Mss. grecs des villes hanséatiques,' Centralblatt für Bibliothekswesen, 1890, p. 361.

G. Genoa, Biblioteca della Missione Urbana, Ms. gr. 37, membr. saec. xi. This manuscript is at present numbered xxxvii (plut. 31. 6. 8), but is given as xxvii in the catalogues of Ehrhard (Centralblatt für Bibliothekswesen, 1893, pp. 204-205) and Bertolotto (Atti della Società Ligure di Storia Patria, vol. XXV, 1892, p. 57), and by Pitra (Analecta Sacra et Classica, V, p. 45).

S. London, British Museum, add. 12150, saec. v. (411 A.D.); Wright, Catalogue of Syriac Manuscripts in the British Museum, II, pp. 631-633.

V. Mt. Athos, Vatopedi 236. See Note 1, above.

C. Paris, Bibliothèque Nationale, Codex Coislinianus 276, membr. saec. x; H. Omont, Inventaire sommaire des manuscrits grecs de la Bibliothèque Nationale, III, p. 167.

R. Berlin, Preussische Staatsbibliothek, Codex Phillipps. 46, membr. saec. xii; G. Studemund and L. Cohn, Die Handschriften-Verzeichnisse der königlichen Bibliothek zu Berlin, p. 15. This manuscript is usually known as Codex Rupefucaldinus, from its former owner, Cardinal Rochefoucauld. It came into the possession of the Berlin Library from the private collection of Sir Thomas Phillipps of Cheltenham. See Migne, P. G., XCVI, pp. 441 ff.

<sup>8</sup> Migne, P. G., XVIII, 1156, n. 31. The words appear in brackets in Lagarde's edition of H.

Latin preserves the text complete, *potestate igitur contrariorum comparatur et acquiritur possessio contrariorum*. These instances, to which others might be added, suffice to show that the Greek text upon which Torres worked was not H; and the steady agreement with the text of G indicates that it, and not the Hamburg manuscript, was his source. This is confirmed by the fact that Torres was otherwise acquainted with G, for, as Pitra observed, G supplied him with the Greek text upon which his translation of Zacharias of Mytilene was based (*Canisius, Antiquae Lectiones*, V, pp. 143 ff.).

In 1859 Paul de Lagarde made two contributions of great importance to the text of these authors. One was an edition of an ancient Syriac version of Titus of Bostra, *Contra Manichaeos*, books i-iv, to which attention had first been called by Cureton in 1848.<sup>9</sup> It is contained in a manuscript at the British Museum (add. 12,150), dated Nov. 4, 411 A.D., and has the unique advantage of preserving the text of all four books. In H not only are there numerous small lacunae, but the end of the third and the whole of the fourth book are missing. Lagarde's other contribution was a new edition of the text of Titus in H, which corrected some errors in Winkler's transcript and introduced the system of paragraphs found in the Syriac.

From the start, the Greek text had presented critical difficulties. Some of these were due to careless copying, but there were also several breaks in the sense which the older editors attempted to remedy either by transposition of the text or by translations more elegant than faithful. The two most noticeable were in Serapion c. 25 (Migne, P. G., XL, 921) and in Titus i.18 (Lagarde, p. 11.4). The second of these occurs after the words, εἰ τοίνυν ἀνάγκη φύσεως πρὸς τὰς προβολὰς αἰδίως ἐφέρετο, ὅσον ἂν τις φαίη τυγχάνειν τὸ ἀφ' οὗ κατεσχέθη διάστημα τοῦ αἰῶνος ἕως νῦν, δῆλον ὡς ἔτι τούτου μυριοπλασίους αἰῶνας, where H completes the sentence with the words, ἀπὸ τῶν μαθητῶν ἐξόριστος ἀπὸ τῆς διδασκαλίας γέγονεν. Lagarde (p. iii) observed that this break of sequence was due to a long interpolation which ended with the words τῇ ἑαυτοῦ τρόπαια, after which the natural sequence of the sentence quoted above was resumed, so that

<sup>9</sup> W. Cureton, *The Festal Letters of Athanasius*, London, 1848, pp. xvi ff.

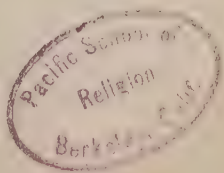
μυριοπλασίους αἰῶνας should be followed by ἡ ἀγένητος ἐγέννα, καὶ χώραν οὐχ ἔξει τὸ καινοτομεῖν τι τὸ αἰδιδιον. The matter which followed τί τὸ αἰδιδιον was evidently in order, but the long interpolation between αἰῶνας and ἡ ἀγένητος remained a mystery. Torres and his successors assumed that it belonged to Titus' work, and attempted to fit it into the text, but their rearrangement was not altogether happy, and the reason for this became apparent when Lagarde showed that the interpolated section was wholly absent from the Syriac and formed no part of the original text of Titus. In his edition of H, Lagarde printed the unidentified interpolation as an appendix at the end of Titus, book iii.

The next discovery was made by Cardinal Pitra. In his *Analecta Sacra et Classica*, V, pp. 44 ff., he drew attention to a manuscript of the eleventh century in the Biblioteca di San Carlo at Genoa which, like H, contained a fragmentary text of Serapion's treatise followed by Titus, books i-iii. As in H, the text of Titus, book iii, was incomplete. Pitra gave a description of the manuscript and its contents, together with a collation of the text of Serapion and Titus made on the printed edition in Migne. He also published some fragments of Titus, found in the recension of John of Damascus's *Sacra Parallela* contained in Cod. Coislinianus 276.

The long interpolation at the end of Titus, book i, which Lagarde had printed as an appendix to book iii, was the starting-point of an investigation by A. Brinkmann.<sup>10</sup>

Brinkmann made three important discoveries: (1) that the text of the interpolated section was not in order, but consisted of five divisions, the seams of which were clearly distinguishable by breaks in the sense; (2) that these divisions could be rearranged to form a long fragment of readable text, the beginning and end of which were missing, and in the middle of which was a gap for which no filling could be found in H; (3) that this fragment could be satisfactorily interpolated in the unintelligible

<sup>10</sup> A. Brinkmann, 'Die Streitschrift des Serapion von Thmuis gegen die Manichäer,' *Sitzungsberichte*, Berlin Academy, 1894, pp. 479 ff. In his edition of Alexander of Lycopolis (Leipzig, 1895), pp. xxx-xxxi, Brinkmann suggests a connection between G and a collection of anti-manichaean writings preserved in a manuscript at Florence (ix-x saec.), *Bibl. Laur. plut.* ix. 23 (Bandini, I, pp. 427 ff.).





sentence of Serapion, *Adversus Manichaeos*, c. 25 (Migne, P.G., XL, 921 C) διὰ τοῦτο οὔτε ἐπιστήμην τοῦ εὐαγγελίου ἔχουσι, τὴν τῶν εὐαγγελίων ἀρχὴν μὴ παραλαβόντες· ἀρχὴ τοῦ εὐαγγελίου Ἰησοῦ Χριστοῦ· καθὼς γέγραπται ἐν Ἱεσαΐα τῷ προφῆτῃ· τὴν ἀρχὴν τῶν μαθημάτων ἐκβεβλήκασι τῇ ἀρχῇ τῶν τῆς πονηρίας ἰστών. The words τῆς πονηρίας ἰστών make nonsense, but the original end of the sentence Brinkmann showed was to be found in Lagarde's appendix to Titus' third book (Lagarde, p. 72. 29), and reads: μαθημάτων ἐμαχέσαντο, τὰ τελευταῖα ἐξήτησαν, εὐρεῖν οὐκ ἠδυνήθησαν, ἀπὸ τῆς ἀρχῆς τὴν ἀκολουθίαν μὴ παραδέξάμενοι. Similarly, the beginning of the sentence ending τῆς πονηρίας ἰστών could be supplied from Lagarde, p. 103. 11 ff., εἰς δὲ μόνος ἔστιν ὁ Χριστός, . . . ὁ ἐν τῇ εἰσόδῳ τὴν φιλίαν τὴν πρὸς τὸν πατέρα ἀποδιδούς, ὁ παρὰ τὴν εἰσόδῳ τῇ ἑαυτοῦ τρόπαια.

Brinkmann was able to strengthen his position considerably by the discovery of a quotation of Lagarde, p. 73.27-32, in Cod. Coislinianus 276, f. 139 (Pitra, p. 48), under the heading, τοῦ ἁγίου Σεραπίωνος ἐκ τῆς κατὰ Μανιχαίων πραγματείας, and by the evidence of G. An examination of the latter furnished the explanation of the confusion in the Hamburg text, for it appeared that the loss of one folium and the displacement of several others in G corresponded exactly with the lacuna and disarrangement in H. These results not only amply justified Brinkmann's analysis of H, but showed, what was more important, that G was the archetype of H for these two treatises,<sup>11</sup> so that H could

<sup>11</sup> This statement should perhaps be qualified, as it is possible that H was derived not immediately from G but from a copy of G. H contains not only the text of Serapion and Titus but also a text of Photius taken from a manuscript at the Vatican (Cod. Vat. Gr. 1923, cf. Brinkmann, p. 485, n. 2). It is natural to suppose that the text of Serapion and Titus was also copied from a manuscript at Rome, and such a manuscript exists at the Bibliotheca Angelica, Cod. Lat. 229, C. 2.15 (Pio Franchi de' Cavalieri and Giorgio Muccio, *Index codicum graecorum bibliothecae angelicae*, Studi Italiani de Filologia Classica, IV, 1896, pp. 170-171). This manuscript is plainly a transcript of G made after the disarrangement of pages of the latter, but the Angelica codex has apparently lost some of its original contents. It is impossible to prove with certainty its relation to H, but καταπεπομένος H (Lagarde, p. 75.25) for καταπεπομένων VG might easily have arisen from the reading of Angelica καταπεπομενος, and ἀνακόλουθα II (Lagarde 72.29), omitted in VG, appears correctly in the Angelica codex as a marginal note. Further investigation of this manuscript might settle the question of its connection with II, but would hardly add to our knowledge of the text of Titus and Serapion.

safely be ignored in reconstructing the text of Serapion and Titus.

With the elimination of H, G has remained since 1894 the only authority for the text of Serapion, *Contra Manichaeos*, and the only Greek witness to the text of Titus' corresponding work. But if the textual problem has in both cases been simple, no one has attempted to solve it.<sup>12</sup> The discovery of Codex Vatop. 236 demands a reconsideration of the whole question and a statement of the results gained from a study of the text of the new manuscript.

## II

### THE TEXT OF SERAPION

For the text of Serapion, V confirms Brinkmann's conclusions about the proper order of the text, and fills the lacuna which he noted after the words ἐξ ἀγαθοῦ προήρχετο (Lagarde, p. 72.29). To Brinkmann's table showing the correct order of the treatise, I have added the corresponding sequence of folia in V.

Migne, P. G., XL, 900c-921c, τῇ ἀρχῇ τῶν<sup>13</sup> = V, f. 38a.1-43a.13.  
Titus Bost., ed. Lagarde, p. 72.29 μαθημάτων<sup>14</sup> to 75.25 ἐκβέβληται = V, f. 43a.13-44a.34.

Titus Bost., ed. Lagarde, p. 69.29 ἀπὸ τῶν μαθητῶν<sup>15</sup> to 72.29 προήρχετο = V, f. 44a. 34-45b.21.

Titus Bost., ed. Lagarde, lacuna in HG = V, f. 45b.21 ταῦτα μὲν to 46a.31 καὶ ἀδιάφορος.

Titus Bost., ed. Lagarde, p. 78.19 καὶ ἐν τούτῳ to 79.37 τὴν κατάποσιν τῶν = V, f. 46a.31-47a.5.

Titus Bost., ed. Lagarde, p. 75.25 καταπεπομένων<sup>16</sup> to 78.19 ὀνόματι = V, f. 47a.5-48a.25.

<sup>12</sup> Neither Brinkmann's edition of Serapion nor one of Titus by Brinkmann and L. Nix, mentioned by Bardenhewer (*Geschichte der altkirchlichen Literatur*, III, 1912, p. 272) as "schon lange in Vorbereitung," has appeared.

<sup>13</sup> The reading τὴν ἀρχὴν appears to be an error of H.

<sup>14</sup> The word ἀνακλόουσα is superfluous and is missing in V. It was probably originally a marginal note.

<sup>15</sup> V reads μαθημάτων.

<sup>16</sup> H reads καταπεπομένης.

Titus Bost., ed. Lagarde, p. 79.37 καλοῦσι to 103. 16 τρόπαια = V, f. 48a. 25–59a.27.

Migne, P. G., XL, 921 C τῆς πονηρίας ἰστών<sup>17</sup> to 924B = V, f. 59a.27–59b.

V is therefore the only known manuscript that preserves a complete text of the treatise.

The relation of V's text of Serapion to that of G can be made out with tolerable certainty. Omissions by homoeoteleuton make it clear that neither manuscript is a copy of the other. In Migne, P. G., XL, 908.7–8, after ἐσωφρόνησεν, V omits the words εἰ δὲ ἐκ ῥαθυμίας ἐπὶ σωφροσύνην μετέστη, καὶ ποτὲ μὲν ἦν ῥάθυμα, ποτὲ δὲ ἐσωφρόνησε, and a similar omission by G is to be found at 909.35 where, after οὐ μένει, V adds ἀλλὰ πολλάκις μὲν σῶμα σωφρονεῖ, πολλάκις δὲ ψυχὴ ῥαθυμεῖ.

It is, however, almost equally certain that both V and G are copies of a single original. The fact that the two treatises appear together, and in the same order, in manuscripts of approximately the same date but of different contents is significant. More positive is the evidence of Serapion c. 12, where, after the words ὑπηρετεῖ γὰρ σωφρονοῦν (Migne, P. G., XL, 909 D), there is an obvious break in the sense which is cured by neither G nor V and which undoubtedly was caused by an omission in their archetype. Furthermore, the textual differences between G and V are slight and insignificant, involving nothing more serious than omission of articles, conjunctions, and pronouns, or unimportant changes of tense. G is perhaps a slightly more accurate copy than V, but it is impossible to follow either manuscript completely in points of detail. Thus in 905.2 G reads wrongly ἐκείνω λόγος for ὁ ἐκείνων λόγος V, and in 916.33–34 has misread οὐκ ἔχεις, ὁ εὖρες for ἔχεις, ὁ εὖρες V, but in 913.21–22, G's ἀνῆρκεν is preferable to V's ἀνῆρηται. In a number of instances the inaccuracy of V is obvious, as in 909.24–25, ἄτοπον for ἄτοποι; 916.11, κατηγορεῖς for κατηγορεῖν; 917.5, ἵνα for εἶναι. In 901, cc. 2–3, the text of H has been badly damaged, and the remedy supplied in Pitra's collation of G is only partly adequate; in V the text of these chapters is in a readable condition.

<sup>17</sup> V adds κατὰ before τῆς πονηρίας.

## III

## THE TEXT OF TITUS OF BOSTRA

The printed text of Titus, no less than that of Serapion, leaves much to be desired. The text in Migne, P. G., XVIII, 1070 ff., is a mass of confusion, and Lagarde's text is at best an accurate copy of H, to which a few happy conjectures have been added. Pitra's work includes a collation of G, made against Migne's edition, together with the evidence of some fragments found in Cod. Coislinianus 276. The collation of G for Titus is less complete than that for Serapion's text,<sup>18</sup> but the evidence is sufficient to determine its relation to V. Like G, V contains Titus, books i-ii and a portion of book iii, but its text of book iii is considerably longer. The text of G breaks off in the middle of a sentence about half way through § 7 (the section numbers here used follow the sections of the Syriac adopted by Lagarde). V and S give the sentence complete, but G stops with the words, οὐκ ἂν ἐπενεχθέντα.

## Text of V (f. 90b).

ὠνείδισε δὲ καὶ τὸν φόνον τοῦ Ἀβελ·  
καὶ ἐπιὼν τὸν κατακλυσμὸν προ-  
βάλλεται ὡς οὐκ ἂν ἐπενεχθέντα  
πρὸς ἀγαθοῦ κατὰ πλασμάτων  
αὐτῆς (sic).

Text of Syriac (Lagarde,  
p. 85.11-13).

ܕܢܫܝܢ ܫܕܕܝܢ ܩܕܝܫܐ ܕܢܫܝܢ  
ܕܢܫܝܢ ܕܢܫܝܢ ܩܕܝܫܐ ܕܢܫܝܢ ܕܢܫܝܢ  
ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ  
ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ  
ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ

V then continues with five more folia containing matter not contained in G, and breaks off shortly after the beginning of § 30.

## Text of V (f. 95b).

ὁ δὲ γε Μάνης καὶ οἱ ἐξ αὐτοῦ  
σκότος ἑαυτοῖς πλάνης περιθέντες,  
ἔτι μείζον αὐτὸ καθ' ἑαυτῶν ἄπερ-  
γάζονται· διὰ τῶν ἀσαφῶν καὶ  
πλείστης ὅσης παρὰ θεοῦ νοήσεώς  
τε καὶ ἐρμηνείας δεομένων· οἴμε-  
νοι δύνασθαι τοὺς ἰδίους στοχασ-  
μοὺς τῆς βλασφημίας βεβαιοῦν·

Text of Syriac (Lagarde,  
p. 98.20-24).

ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ  
ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ  
ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ  
ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ  
ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ  
ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ  
ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ ܕܢܫܝܢ

<sup>18</sup> Pitra, p. 59, n. 3. Quae varietates sat multae colliguntur, sed non omnes, quia urgebat tempus a codice recedendi, tum maxime quod solus huic operi accingebar, in quo unus hinc inde distractus oculus, interdum coecus est.

At this point the Syriac continues the argument, and book iii comes to an end some thirty pages later; but V brings the book to an abrupt conclusion with the words, ἀλλ' ἐσφάλησαν οἱ παράφρονες τῆς ἀληθείας ἀποπλανηθέντες καὶ τῷ σκότει προσπελάσαντες· οὐ καὶ κληρονόμοι γενήσονται, ἀξίους ὧν ἐτόλμησαν τοὺς καρποὺς δρεψάμενοι. ἡμῖν δὲ δώῃ ὁ θεός· ἀκυμάντους διατηρηθῆναι καὶ ἀθιγεῖς τῆς αὐτῶν βδελυρᾶς καὶ κακίστης αἰρέσεως. καὶ πάντων ἄλλων, οἳ κατὰ τῆς δόξης τοῦ θεοῦ ἐπήρθησαν. φενάκει λογισμῷ, τοῦ ὅφους καὶ αὐτοὶ ἀπατηθέντες, ὡς ἐᾷ (sic) τὸ πρῖν.

To judge by Pitra's collation of Titus in G, V would exhibit a considerably larger number of 'singular' readings than was the case in Serapion's treatise, but this is probably due only to the fact that Pitra's collation is less complete for Titus than for Serapion. In a number of instances V G combine to correct the imperfect text of Basnage and Migne, but a comparison with Lagarde's edition of H shows that in more than one case the source of difficulty was not H but the transcription of it, supplied to Basnage by the friendly Winkler. Thus in Migne, XVIII, 1136.1 ff., the unreadable text of Basnage has been emended to approximate to the sense of Torres' translation. Lagarde's edition of H (pp. 27.12 ff.) shows a text substantially identical with that of V G.

A parallel case occurs in Migne, XVIII, 1168.53, where after ἕως μεσημβρίας ἥλιος, V G add μετὰ μεσημβρίαν σκιά. οὐ δὲ σκιά μεταμεσημβρίαν, ἥλιος, which agrees with Lagarde's text of H (p. 45.31-32). Also in Migne, XVIII, 1205.36, an unreadable text is given which a comparison with Lagarde, p. 66.3-4, shows to be due to careless copying. With the true text of H, V and G are in substantial agreement.<sup>19</sup> In Migne, XVIII, 1156.2, however, V G add, after τοὺς μὲν ἄνευ τροφῆς, the words διαζῶντας τοὺς δὲ διὰ τροφῆς, which are missing in H (Lagarde, p. 38.11).

In several passages the evidence is complicated by quotations from Titus in the Sacra Parallela of John of Damascus. These quotations are found in the Codex Rupefucaldinus and in Cod. Coislinianus 276, and three of them exist, with slight

<sup>19</sup> Pitra omits, by homoeoteleuton, σύνδρομον αὐτῷ τὸ εἶναι, but this is clearly his own mistake. V reads αὐτό for αὐτῶν before πρὸς τὸ εἶναι.



textual differences, in both manuscripts.<sup>20</sup> Pitra has published the fragments peculiar to Coisl. 276 and a collation of two fragments that are contained in both manuscripts. Before using these quotations, however, it is necessary to correct numerous errors in the transcription and collations of Pitra. In *Analecta*, V, pp. 52–54, he gives a text which is quoted also in *Rupeguladinus* (Migne, P. G., XVIII, 1257–1260; XCVI, 486). It appears in Coisl. 276, fol. 88b. 7–89a. 30, under the caption τοῦ μακαρίου Τίτου ἐπισκόπου Βόστρον, ἐκ τοῦ κατὰ Μανιχαίων  $\bar{\alpha} \bar{\delta}$ . The following corrections of Pitra's transcription are to be noted:

p. 52, last line, οὐ τό for οὐδέ

p. 53.3 add τῶν before παθῶν

4 ἐπαπορήσεως

14 χόρος altered to χῶρος

18 δέμενον for δεομένῳ

35 add ἦν ἂν ἄλογος after ψυχῇ

37–38 σημένει (altered by later hand to σημαίνει)

39 ἀφθαρσίαν for ἀθανασίαν

p. 53.40–54.1 συγκαμόντος for συγκαμόντο

p. 54.4 add μὲν after ἀπειληφύας

9 συμπονήσαν for συμπονήσαι

10 add τὴν after τήνδε

11 ἐπάθλων for ὑπάθλων

*Analecta*, V, pp. 54–56, gives a text which Pitra says is found in Coisl. 276, f. 82. This is a mistake, for the passage actually occurs not on f. 82 but on ff. 207a.14–208a.9. It is introduced by the caption τοῦ ἁγίου Τίτου ἐκ τοῦ περὶ Προνοίας B, but is a quotation from Titus, book ii. c. 21 (Migne, XVIII, 1153.11–1156.20; Lagarde, p. 37.17). The following corrections of Pitra's transcription are to be noted:

p. 54.18 ἀποκέκλεισται for ἀποκέκλειται

22 κοινόν for τοίνυν

24 καταφωράσαιεν

29 τροφείοις (sic)

<sup>20</sup> A study of these two manuscripts in relation both to the original form of the catena they contain and to the textual value of the patristic quotations they supply, has been made by Holl, *Die Sacra Parallela des Johannes Damascenus* (Texte und Untersuchungen, N. F. I. 1), Leipzig, 1897.

- p. 55.1 after αἰσθήσεως add ἁμαυρομένης· μηδ' ἄλλον τοῦ ἐλάττονος καὶ τούτου διὰ τῆς συνηθείας (cf. Pitra, p. 55, n. 1)
- 4 ἄτρωτοι (corrected in margin to ἄστρωτοι, not ἄστροτοι)
- 9 οἶοι τε ὄντες/δυνατοὶ ὄντες in margin
- 10 ἂν for οὖν
- 10-11 ταχέως in margin for θαττον (?)
- 11 κρυμούς for κρυμνούς
- 14 πλέον for πλείον
- 19 MS. reads διαζώντας, not διαξώντας (Pitra, p. 55, n. 4)
- 28 MS. reads ἄνευ ζῆσαι, not εὐξῆσαι (Pitra, p. 55, n. 5)
- 35 no indication that μηδαμῶς belongs after εἰ γάρ
- 35 om. ἡ before διαφορά
- 37 τεχνήτη (sic) for τεχνίτη
- 37 ἀρμόττων for ἀρμόττον
- p. 56.3 αἰσθήσεις for αἰσθήσεσι

Analecta, V, p. 56, gives a passage found in Coisl. 276, ff. 214b.10-215a, under the caption, τοῦ ἁγίου Τίτου ἐκ τοῦ περὶ Προνοίας. It is a quotation from Titus, ii. c. 23 (Migne, XVIII, 1180.45-1181.10; Lagarde, p. 52.7 ff.). The following corrections of Pitra's transcription are to be noted:

- p. 56.9 τούτου for τούτων
- 11 καταδεξαμένου for καταδεχομένου
- 18 ἂν εἴη. αὐτίκα, οὐδὲ
- 26 ἐθέλει for ἔθελε

In Analecta, V, p. 57, a brief passage appears from Coisl. 276 (ff. 252a.23 ff.), where it is introduced by the caption τοῦ ἁγίου Τίτου ἐκ τοῦ περὶ Προνοίας. This passage Pitra attributes, without the authority of the manuscript, to Titus, book ii.

In Analecta, V, p. 63, Pitra gives a collation of Coisl. 276 for two passages which appear in Rupefucaldinus. The first is Coisl. 276, ff. 215a.2-25 (Migne, XVIII, 1261.25 ff.).<sup>21</sup> It is

<sup>21</sup> Pitra's figures, "Cod. fol. 214 f. Patrol. col. 1261 A. 1. 14," are wrong.

introduced by the caption, ἐκ τοῦ αὐτοῦ, and contains the following variants unnoticed by Pitra:

- 1261.24 add δὲ after ἐχρῆν  
 25 add τῆς before τοῦ θεοῦ  
 28 ὅτε for ὅτι  
 29 δ τῷ for ὅτω  
 30 add γε after ἀνθρωποι  
 30 ὅλως/ὅλης in margin  
 33 μηδαμῶς in margin for ἡκιστα (?)  
 33 ἀντερεῖν for ἀνταίρειν  
 34 ἦτουν for εἴτουν  
 38 σφαλλομένων for σφαλλομένω  
 39 εἵπομεν for εἵπομι  
 44 λίαν for πάλιν

The collation of Coisl. 276, fol. 258b.19–259a.10 (Migne, P. G., XVIII, 1261.4–23), in *Analecta*, V, p. 63, is correct, except at 1261.11, where Pitra gives γὰρ οὖν for τοιγαροῦν although the manuscript reads τοιγαροῦν. The passage is a quotation from Titus ii (Migne, XVIII, 1164.38, Lagarde, p. 43.18 ff.), and is introduced by the caption τοῦ ἀγίου Τίτου ἐκ τοῦ περὶ Προνοίας β̄.

#### IV.

#### CONCLUSIONS

With the evidence now in hand the task of an editor of these two treatises is a relatively simple one.<sup>22</sup> It is plain that the evidence of G must be re-examined, for Pitra's collations supply hardly more than useful illustrative material, which is often deceptive in its incompleteness and occasionally entirely wrong. For the text of Serapion, V and G are the only witnesses, but for Titus the evidence is complicated by the Syriac and the fragments from Codex Rupefucaldinus and Coisl. 276.

These fragments are of small importance, but in the Syriac singular readings deserve special consideration, and in cases of

<sup>22</sup> An edition of both texts by Professor F. C. Burkitt and myself will, it is hoped, form a volume of the Harvard Theological Studies.

divergence between V and G agreement with the Syriac must be the decisive factor.

An example of this occurs in Titus, ii.27 (Lagarde, p. 43. 25-26, Migne XVIII, 1164.49), where the text of V reads *γινώσκοντες ὡς αὐτὸς γέ ἐστιν ὁ παρέχων καὶ οὐ φύσις ἄλλη τίς παρὰ τὸν θεὸν ἄκλητὸς τε καὶ ἀμετάκλητος ἢ χορηγούσα*. In G the adjectives modifying *φύσις* are *ἄκριτὸς τε καὶ ἀμετάβλητος*, which are supported by ܐܠܡܳܘܽܬ ܕܠܳܗܳܐ ܚܳܝܳܐ ܕܳܐ of the Syriac<sup>23</sup> and by intrinsic probability.

Similarly, in Migne, XVIII, 1188.16-18 (Lagarde, p. 56.35), G reads *τί δ' ἂν εἴποιμεν περὶ τῆς ὑπὲρ ἅπαντα νοῦν σοφίας τοῦ θεοῦ, ἣν προσῆκε νοοῦντας καὶ μὴ νοοῦντας ἐκπλήττεσθαι τε καὶ τιμᾶν*; Here V wrongly omits *ἣν* before *προσῆκε*, although it is required by the sense and appears not only in G but in the Syriac (p. 70.27).

In dealing with the evidence of the Syriac some caution must be observed. The Greek of Titus is neither easy nor graceful; but the Syriac of his translator is excessively pedantic, and a desire for accuracy has led him to construct sentences which, without the aid of the original, are sometimes barely intelligible.<sup>24</sup> His care to render everything often facilitates the control of minor variants, but he sometimes paraphrases words and expressions of the Greek for which he can find no Syriac equivalent exact or complete enough for his purpose. A particularly confusing example of this is to be found in Titus, ii. 48 (Lagarde, p. 56.23-25), where the text of V reads *οὐ γὰρ ἐχρήην γε, ὡς ἔοικεν, κατὰ τοῖς πᾶσιν ὑπάρχειν τῆς θεοῦ σοφίας τὰ τεκμήρια, καὶ τὸν ἄρρητον καὶ ἀκατάληπτον νοῦν πάντη γυμνὸν εἶναι ἀνθρώποις*, with which G is in substantial agreement. In the Syriac (Lagarde, p. 70.12-13) *τὸν ἄρρητον καὶ ἀκατάληπτον νοῦν* is rendered: ܐܠܡܳܘܽܬ ܕܠܳܗܳܐ ܕܳܐܪܳܪܳܬܳܐ ܕܳܐܠܳܗܳܐ ܕܳܐܠܳܗܳܐ. To one unfamiliar with the habits of the translator, the underlying Greek would appear to be *τὸν κρυπτὸν καὶ ἀκατάληπτον καὶ ἄρρητον νοῦν*, but a few lines farther down (p. 56.31) *κατὰ τῆς ἀρρήτου διοικήσεως* is rendered (p. 70.20) by ܐܠܡܳܘܽܬ ܕܠܳܗܳܐ ܕܳܐܠܳܗܳܐ ܕܠܳܗܳܐ ܕܠܳܗܳܐ ܕܠܳܗܳܐ. Here

<sup>23</sup> C agrees with G S, but R reads *ἀμεταμέλητος*, which is evidently a mistake for *ἀμετάβλητος*, arising from a misreading of *μ* for *β*.

<sup>24</sup> The first sentence in Titus, book i, is a good example of both styles.



ܐܠܗܐ merely strengthens ܐܠܗܐ ܐܠ, as above it strengthened ܐܠܐܠܐ ܐܠ. It is probable that in the first instance the Greek underlying the Syriac read τὸν ἀκατάληπτον καὶ ἄρρητον νοῦν, but the presence of the awkward ܐ ܐܠܗܐ does not imply a third adjective in the Greek.<sup>25</sup>

<sup>25</sup> Examples of this phenomenon abound. Cf. Lagarde, Greek, p. 56.24 τὰ τεκμήρια = ܐܠܗܐ ܐܠܗܐ (Lagarde, Syriac, p. 70.11); Greek, 56.27 τῶν βουλευμάτων = ܐܠܗܐ ܐܠܗܐ (Syriac, 70.15); Greek 56.39 ἐκπέσοι = ܐܠܗܐ ܐܠܗܐ (Syriac, 70.3).



